THE

EDINBURGH NEW
PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE
PROGRESSIVE DISCOVERIES AND IMPROVEMENTS
IN THE
SCIENCES AND THE ARTS.

CONDUCTED BY

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APRIL...OCTOBER 1835.

VOL. XIX.

TO BE CONTINUED QUARTERLY.

EDINBURGH:
ADAM & CHARLES BLACK, EDINBURGH;
LONGMAN, REES, ORME, BROWN, GREEN & LONGMAN, LONDON.

1835.

BRITISH MUSEUM
NATURAL HISTORY

PRINTED BY NEILL & COMPANY,
OLD FISHMARKET.

CONTENTS.

ART. I. Physico-Geognostic Sketch of the Island of Oahu, one of the Sandwich group. By MEREDITH GAIRDNER, M. D., Medical Resident on the Columbia River, ... Page 1

General Sketch of Formations, . . . 8

II. On the Number of Comets in the Solar System; on their Light, whether Reflected or Emitted; and on the Comet which will pass its Perihelion in November 1835. By M. ARAGO, . . 15

1. On the Number of Comets, ... 15
2. On the Light of Comets; and on the means of deciding whether this light emanates from these bodies themselves, or is borrowed from the Sun. . . 21
3. On the Comet which will pass its Perihelion in November 1835, ... 31

III. On the periods of the Deluges of Deucalion, Oxyges, and Noah, according to the various calculations of time of different Nations. By Dr VON SCHUBERT, ... 34

IV. Note respecting the application of the Compressibility of Water to Practical Purposes. By JAMES D. FORBES, Esq. F.R.SS. L. & E., Vice-Pres. Soc. Arts, and Professor of Natural Philosophy in the University of Edinburgh, . . . 36

V. Upon the Egyptian Hieroglyphical Writing. By M. ARAGO, ... 39

VI. Remarks on the Garnkirk and Glasgow Railway. By Mr GEORGE MARTIN, Edinburgh, . 48

VII. Account of some of the Rarer Plants observed during an Excursion in the United States and the Canadas in 1834. Communicated by Mr JAMES MACNAB, ... 56

VIII. Remarks on the question, Does the observation made in Coal-mines of Canada and of Baffin's Bay, of plants analogous to those which live at present in equatorial regions, announce a change in the inclination of the Ecliptic? By M. MARCEL DE SERRES, ... 64

IX. Account of the Founding of a New Grand Observatory for the Russian Empire, by the Imperial Academy of Sciences of St Petersburgh, . 71

X. Account of some remarkable Hailstones which fell at Padua, on the 26th of August 1834. By D. L. COSARI, ... 83

Hailstones of a Flattened and Irregular Shape, . . 84

Hailstones of a Spherical Form, ... 85

XI. Account of an attempt to ascend Chimborazo, made on the 16th December 1831. By J. B. BOUSSINGAULT. In a letter addressed to Alexander Von Humboldt, ... 88

XII. On the Characters of certain groups of the Class Personatæ. By DAVID DON, Esq. Libr. L. S., &c. 108

XIII. Account of the Discovery by Purkinje and Valentin of Ciliary Motions in Reptiles and Warm-blooded Animals; with Remarks, and additional Experiments. By WILLIAM SHARPEY, M. D., F. R. S. E., Lecturer on Anatomy, . . . 114

Discovery of Continual Vibratory Motions produced by Cilia, as a general Phenomenon in Reptiles, Birds, and Mammiferous Animals. By Professor PURKINJE and Dr VALENTIN, of Breslaw, . 118

Parts of the Body, and Classes of Animals, in which the Vibratory Motions exist, ... 119

Method of Investigation, ... 121

Nature and Character of the Vibratory Motions, . 122

Additional Observations and Experiments, by Dr SHARPEY, ... 125

XIV. On the Manner in which Friction affects the Motions of Time-Keepers. By Edward Sang, Esq. Teacher of Mathematics, Edinburgh, Member of the Society of Arts, &c. 129

XV. Progressive Increase of Temperature as we descend in the Crust of the Earth, . . . 143

XVI. Researches on Wines and other Fermented Liquors. By Lewis C. Beck, M.D., Professor of Chemistry and Botany in the University of the City of New York, &c. &c. 145

XVII. On the Action of Voltaic Electricity on Alcohol, Ether, and Aqueous Solutions. By Arthur Connell, Esq. F.R.S.E. . . . 159

XVIII. Memoir on the Degree of Selection exercised by Plants, with regard to the Earthy Constituents presented to their absorbing Surfaces. By Charles Daubeny, M.D., F.R.S., L.S., G.S., Professor of Botany and Chemistry in the University of Oxford, ... 164

XIX. Note on Taking the Dew-Point directly, by the Method of Leroi and Saussure, . . . 177

XX. Scientific Intelligence, . . . 189

NATURAL PHILOSOPHY.

1. Velocity of Electricity, . . . 179

GEOLOGY.

2. On the Imprint of the Feet of Animals upon Red Sandstone. 3. Rolling of the Sea on the Coast of Peru. 4. Large Mass of Native Copper. 5. Earthquakes. 6. Fossil Marine Plants. 7. Gold Region of the United States, . . 182–185

ZOOLOGY.

8. On Aphis Persicæ. 9. Edible Birds Nests, how formed. 10. Pilot-fish. 11. Effect of Mountain Air on Man. 12. Temperature of Fishes. 13. Salmon of the Columbia River. 14. Respiration of Birds. 15. On the Mytilus polymorphus, or the Driessena polymorpha, a remarkable species

of Mussel. 16. Discovery of Remains of the Mammoth in India, . . . 188–196

ETHNOGRAPHY.

17. Projected Expedition to Iceland and Greenland, 196

XXI. Description of several New or Rare Plants which have Flowered in the Neighbourhood of Edinburgh, chiefly in the Royal Botanic Garden. By Dr GRAHAM, Prof. of Botany, . . 202

XXII. Proceedings of the Royal Society of Edinburgh, 203

On the History of the Arch, by Dr TRAILL, 203. On the Application of the Hot Blast in the Manufacture of Iron, by Dr CLARK, 205. On the Ruins of Babylon, by Dr TRAILL, 206. On the Expansibility of Stone, by Mr ALEX. ADIE, Civil-Engineer, 207.

XXIII. Proceedings of the Wernerian Natural History Society, ... 208

Mr CUNNINGHAM on the Geology of the Island of Skye; and Professor JAMESON on the Birds of the Himmalayan Mountains, &c. 208. Mr PARNELL's Account of some New Species of Fishes found in the Firth of Forth, 209. Professor JAMESON on a New Species of Eagle, and male Lophophorus Nigelli, 212. Mr JAMES WILSON on Insects of Sutherlandshire, 212. Professor JAMESON on New Species of Ibis, Tanagra, and Rubecola, 213.

XXIV. Proceedings of the Society for the Encouragement of the Useful Arts in Scotland, . . 214

XXV. List of Patents granted in Scotland from 25th September 1834 to 10th June 1835, . . 217

CONTENTS.

ART. I. On the Volcanic Strata exposed by a Section made on the site of the New Thermal Spring discovered near the Town of Torre del Annunziata, in the Bay of Naples; with some Remarks on the Gases evolved by this and other Springs, connected with the Volcanos of Campania. By CHARLES DAUBENY, F.R.S. and F.G.S., Professor of Chemistry, Oxford. Communicated by the Author, - - - - - - Page 221

II. Inquiry into the Changes which the Atmosphere undergoes when in contact with certain Vegetables which are destitute of green leaves, &c. By M. F. MARCET, - - - - - 232

Section I. On the Action of Mushrooms on Atmospheric Air, - - - - - - 235

II. On the Action of Mushrooms upon pure Oxygen Gas, - - - - - - 240

III. On the Action of Mushrooms on pure Nitrogen Gas, - - - - - - - 242

III. On the Distinctive Characters of the Dog, the Wolf, and the Fox, as supplied by the Skeleton. By M. MARCEL DE SERRES, - - - 244

ART. IV. First Annual Report regarding New Inventions and Improvements in the Useful Arts throughout Europe; ordered by the Society of Arts for Scotland: Being a Report on the recent Improvements of the Carpet Manufacture. By Mr EDWARD SANG, Teacher of Mathematics, and Lecturer on Natural Philosophy, Edinburgh, M. S. A. 254

V. On the Vegetables which are found in the Thermal Mineral Waters of Abano, and in their immediate vicinity. By Dr ANDREJEWSKYI, - 261

VI. On the Animals designated in the Scriptures by the Names of Leviathan and Behemoth. Communicated by the Author, - - - - 263

VII. On the Falls of Niagara, and the Reasonings of some Authors respecting them. By HENRY D. ROGERS, F. G. S. of London, &c. - - 281

VIII. Remarks on the Distribution of Insects, and Account of Coleoptera taken in Sutherland in June 1834. By JAMES WILSON, Esq., F. R. S. E., M. W. S., &c. Communicated by the Author, 292

IX. On the Chemical Nature of Fossil Scales, as illustrative of the Character of the Animals to which they have belonged. By A. CONNELL, Esq., F.R.S.E., &c. Communicated by the Author, 300

X. Notice of an Arrangement for the Measurement of the Angles of very Minute Crystals. By the Rev. EDWARD CRAIG, A. M., Oxon., F. R. S. E., Councillor of the Society of Arts, - - - 306

XI. Description of a New Spring for Shutting a Door, which goes both ways. By ALEXANDER BEATTIE, Foreman to Mr ROBERT RITCHIE, Ironmonger to the King, 241 High Street, Edinburgh, - - - - - - - 309

Report of Committee of Society of Arts on Mr Beattie's Spring Door, read 9th April 1834, with a Plate, 310

XII. On the different Chronology of the Deluge, according to the Hebrew Text and the Septuagint Translation. In a letter to Professor JAMESON, by Mr JAMES LAURIE, Edinburgh, - - 311

ART. XIII. Notice of the Remains of a Fish found connected with a Bed of Coal at Clackmannan. In a Letter to Professor JAMESON from Dr FLEMING, Professor of Natural Philosophy in King's College, Aberdeen. With a Plate, . - - 314

XIV. Remarks on Certain Statements of Mr Faraday, contained in the Fourth and Fifth Series of his "Experimental Researches on Electricity." By JOHN DAVY, M.D., F.R.S. Communicated by the Author, - - - - - - 317

XV. On the Temperature of some Fishes of the genus Thynnus. By JOHN DAVY, M.D., F.R.S., Assistant-Inspector of Army Hospitals. Communicated by the Author, - - - 325

XVI. Professor AGASSIZ on the Arrangement and Geology of Fishes, . . - - - 331

XVII. Notice of Plants new to the British Flora, or rare in Scotland, observed during the last Twelve Months, in various Excursions from Edinburgh. Communicated by Dr GRAHAM, - - - 348

XVIII. The British Association at Dublin, in August 1835, 353

SECTION A. MATHEMATICS AND GENERAL PHYSICS, 352.—1. Professor Whewell on the application of Mathematical and Dynamical Principles to Magnetism, Electricity, Heat, &c. 2. Mr Snow Harris's description of a new instrument for measuring Electrical Forces. 3. Professor Powel on Radiant Heat. 4. Dr H. Hudson on the Radiation of Heat. 5. Captain Ross on a new theory of the Aurora Borealis. 6. Mr R. Mallet on an economic application of Electro-Magnetic Force to manufacturing purposes. 7. Professor Whewell on Heat. 8. Dr Allmann on the forms of the Cells of Plants. 9. Dr Reid on Sound. 10. Mr S. Harris on the nature of Electrical Attraction. 11. Mr J. S. Russel on the laws of the motions of Floating Bodies. 12. Mr Pritchard's Experiments on Polarization. 13. Mr S. Harris on the use of the Proof, Plain, and Torsion Balance of Coulomb and others. 14. Captain Sabine on Hansteen's Researches on Terrestrial Magnetism. 15. Professor Wheatstone on the Light formed by the Voltaic Spark between Charcoal Points. 16. Rev. Mr M'Gauley on the application of Magnetism as a Moving Power. 17. Mr R. W. Fox on a new Dipping Needle. 18. Mr Hamilton on the theory of Logologues, and other numbers of higher orders. 19. On an apparatus by which Figures may be seen, or Sentences read, as distinctly when revolving at a high velocity, as when in a state of quiescence. 20. Mr Baily on the Aberdeen Standard Scale of 3 feet. 21. Mr S. Harris on Thermometric Observations. 22. Mr G. Jerrard on the solution of Algebraic Equations. 23. Mr Phillips on the Fall of Rain. 24. Colonel Sykes on Measuring Heights by the Thermometer. 25. Mr M'Cullagh on Reflection and Refraction of Polarized Light at the surfaces of Crystals. 26. Mr M'Gauley on Magnetism. 27. Professor Apjohn on the Dew Point. 28. Professor Hamilton on Mr Challis's results respecting the simultaneous vibrations of a Cylindrical Tube, and the Column of Air contained in it. 29. Professor Wheatstone on Speaking Machines. 30. Mr Whewell on a new Anemometer. 31. Professor Lloyd's Magnetic Observations. 32. Mr Hamilton on the theory of Varying Orbits. 33. Mr Kane on the Interference of Sound.

MECHANICAL SCIENCE APPLIED TO THE ARTS, 368.—1. Mr Hodgkinson on the Collision of Beams and Piles. 2. Mr Mallet on the Fracture of Bars of Cast Iron. 3. Mr Ettrick on a new Compass. 4. Mr Pritchard on an Achromatic Microscope. 5. Mr Russell on the Solids of least resistance, in reference to Steam-Vessels. 6. Mr John Taylor on the quantity of Coal required for Steam-Engines, &c. 7. Dr Lardner on Rail-Roads. 8. Mr Ettrick on certain improvements in Steam-Engines, &c. 9. Mr Cheverton on the Sculpture of Busts by Machinery. 10. Mr Grubb on an improved model for mounting an Equatorial Instrument. 11. Lieut. Denham on the Vibratory effects of Rail-Roads. 12. Mr Stevelly on a new Self-Registering Barometer.

SECTION B. GEOLOGY AND GEOGRAPHY, 372.—1. Mr Griffith on the Geology of Ireland, accompanied with a geologically coloured map. 2. Dr West on the Geography of some parts of Greenland. 3. Archdeacon Verschoyle on a system of Trap-Dikes passing through the counties of Mayo and Sligo. 4. Mr Phillips on Fossil Astacidæ. 5. Lieutenant Stotherd on the Granite in the county of Cavan. 6. Mr Griffith on the Unstratified Rocks of Ireland. 7. Professor Phillips on the Geological Distribution of Belemnites. 8. Lieutenant Denham, R. N., remarks on a Map illustrative of the Estuaries of the Dee and Mersey. 9. Mr Williams on the Fossil Plants collected in the coal districts of Devonshire and Pembrokeshire. 10. M. Agassiz on the general results obtained from the inspection of the greater part of the collections of Fossil Fishes in Great Britain, and his remarks on a Fossil Fish exhibited by Captain Portlock. 11. Dr Traill on the Geology of Spain, and Mr Greenough on the connexion of the Alps and Pyrenees. 12. Mr Smith on some Fossils found in a quarry near Glasgow. 13. Mr Whewell on various points in Physics connected with Geology. 14. Mr Hartop on the faults in the Southern Division of the West Riding of Yorkshire. 15. Mr Murchison and Professor Sedgwick on the Older Rocks of England and Wales. 16. Dr Jacob on Fossil Madrepores. 17. Mr Phillips on a Tertiary Deposit near Burlington, on the north coast of England. 18. Mr Lyell on the Fossils of the Crag.

SECTION C. CHEMISTRY AND MINERALOGY, 391.—1. Mr Davy on the Corrosion of Iron by Sea Water. 2. Mr Ettrick on an Improvement in Davy's Safety Lamp. 3. Mr Kane on Pyroxylic Spirit. 4. Mr Fox on the effect of Melted Iron upon the Magnet. 5. Dr Turner's Report on the Opinion of the Committee appointed at last Meeting to take into consideration the adoption of a uniform Set of Chemical Symbols for this Country. 6. Mr Davy on the Preservation of Tin-Plate by the Agency of Zinc. 7 Mr Graham on the Constitution of Salts. 8. Mr Johnston on the Optical Properties of Chabasite. 9. Dr Daubeny on the Sublimation of Carbonate of Magnesia by Volcanic Action. 10. Dr Dalton on the Distilled Liquors from Caoutchouc. 11. Mr Mallet on the Holes in Lamps for the Passage of Gas. 12. Mr Connell's Analysis of Fossil Scales. 13. Mr Kane on two Compounds of Tin and Platinum. 14. Mr Snow Harris on a Modified Electrometer. 15. Dr Newbigging on the Colour of Arterial Blood. 16. Mr Hartop on the use of the Air Blast in the Manufacture of Pig-Iron. 17. Colonel Colby on the Ordnance Survey of the County of Londonderry. 18. Dr Apjohn on a Method of Determining the Specific Heats of Gases. 19. Dr Dalton on Chemical Symbols. 20. Mr Mallet on a White Material fitted for the Manufacture of Paper prepared from Turf. 21. Mr Davy on the relative Values of Virginian and Irish Tobacco. 22. Mr Scanlan on a New Fluid, Prepared from Pyroligneous Acid. 23. Mr Moor on the Corrosion of Lead Pipes. 24. Dr Barker on a New Mode of Separating the Peroxide of Iron by means of Acetate of Potash. 25. Dr Geoghehan on the Employment of the Double Salt of Iodide of Potassium and Bicyanide of Mercury, for the purpose of detecting Muriatic Acid in Prussic Acid. 26. Mr Johnston on the Iodides of Gold. 27. Dr William Barker on the Passage of Electricity along a Platinum Wire. 28. Mr Scanlan on Crystallized Hematine.

Section D. Zoology and Botany, 400.—1. Mr Niven on a Natural Arrangement of Plants for a Botanical Garden. 2. Mr Bell on Pentacrinus europæus and Beroe ovatus, in a Living State from Howth. 3. Notice of some Rare Plants found in Scotland. 4. Mr Mackay on Bog Timber from the County of Westmeath. 5. Colonel Sykes on certain Species of Birds common to India, the Cape, and Europe. 6. Mr Nicol on the Structure of the Horizontal Branches of the Natural Family of Coniferæ. 7. Dr West on the Bog Yew. 8. Dr Jacob on the Infra-Orbital Cavities or Larmiers of French Authors. 9. Dr Daubeny on the Circumstances affecting the Exhalation of Moisture from the Leaves of Plants. 10. Dr Neill on a Land-Rail found in a Torpid State. 11. Mr Drummond on the Zoology of the Island of Rathlin, on the Coast of Antrim. 12. Observations on Bog-Tallow. 13. On the Disappearance of many Fishes from the Coasts of Antrim and the Firth of Forth. 14. Mr Sturge on a Live Toad found in a Solid Mass of Red Sandstone. 15. Dr Barry on the Colour of the Sky, as seen from the Summit of Mont Blanc. 16. Mr Hamilton on a Yew found in a Bog in Queen's County. 17. Mr Mackay's List of Plants peculiar to Ireland. 18. On the Localities of Plants. 19. Mr Saunderson on Yew-Trees in the North of Ireland. 20.

Rev. C. Mayne on Echini and Asteriæ. 21. Professor Agassiz on the Principles of Classification in the Animal Kingdom, and among the Mammalia in particular.

Section E. Medical Science, 408.—1. Dr Graves on Chloride of Soda in Fever. 2. Dr Houston on certain peculiarities of the Circulating Organs in Diving Animals. 3. Report of the Dublin Committee on the Motions and Sounds of the Heart. 4. Dr M'Donnell on the Pulse and Breathing in early life. 5. Professor Harrison on the small Bones found in the Hearts of certain Ruminantia. 6. Dr Houston on Hydatids. 7. Professor Harrison on the Entozoa found in the voluntary Muscles of the Human Subject. 8. Dr Roe on the case of a living Leech discharged from an Abscess opened in the Leg of a Boy. 9. Dr Collins on an Abstract of a Registry of seven years in the Lying-in Hospital of Dublin. 10. Dr Roupell on Poisons. 11. Dr Alison on Arteries leading to Inflamed Parts. 12. Dr Whatton on Amputation of the Foot. 13. Dr Stokes on Diseases of the Chest. 14. Dr E. Kennedy on the Treatment of Ophthalmia in Children. 15. Mr Hart on a successful case of Cæsarean operation, communicated by Mr Knowles. 16. Dr Perry on Typhus Fever. 17. M. L'Estrange on an improved Drill Catheter for Calculus. 18. Mr Hawkins on his Patent Electriser. 19. Dr O'Bierne on the Functions and Diseases of the Digestive Organs. 20. Dr Osborne on the effects of Cold on the Human Body. 21. Dr Hutton on a Congenital Dislocation of the Hip. 22. Mr Adams on Aneurism by Anastomosis. 23. Dr Handyside on the different offices of Lacteals, Lymphatics, and Veins, in the Function of Absorption.

Section F. Statistics, 412.—1. Dr Mansell on the Statistics of the Dublin Foundling Hospital. 2. Dr Cleland on Prison Discipline. 3. Colonel Sykes on the new Colony of Australia. 4. Mr Langton on the Report of the Manchester Statistical Society on the state of Education in Manchester. 5. Mr Greg on the social Statistics of the Netherlands. 6. Mr Halsewell on the effects which well-managed Houses of Correction have in repressing Crime. 7. Dr Cleland on the Glasgow Bridewell. 8. Colonel Sykes on Wages in the Deccan. 9. Mr Babbage on Co-operative Shops, for the purpose of supplying Workmen with Necessaries. 10. Dr Mansel on the number of Infanticides in the province of Leinster, drawn up by Dr Vignolles. 11. Colonel Sykes on the state of Education in the Deccan. 12. Rev. E. Stanley on the Religious Attendances and State of Education in the parish of Alderley, in Chesshire. 13. Dr Reid's Views upon a Plan tried in Edinburgh for the extension of the study of Physics. 14. Mr Babbage on the Ordnance Survey of the city of Londonderry and parish of Templemore. 15. Dr Jones on the Statistics of Lunacy. 16. Mr Fox on the Punishment of Death in Russia, Norway, Belgium, and Brunswick.

Evening Meetings in the Rotundo, 416.—1. Sir Thomas Brisbane's Address. 2. Dr Lloyd's Address. 3. Professor Hamilton's Address. 4. Professor Whewell's Address. 5. Mr Harcourt's Address. 6. Banquet at the University.

THE

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Physico-Geognostic Sketch of the Island of Oahu, one of the Sandwich group. By MEREDITH GAIRDNER, M. D., Medical Resident on the Columbia River. Communicated by the Author.[*]

THIS island is the fifth of the group of eight islands, which form the Sandwich cluster, reckoning from Hawaii, the most south-eastern. It is separated by a channel twenty-four miles wide from the island of Monokai on the south-east, and by one sixty-seven miles broad from Tawai on the north-west. It is the third in rank in point of superficial extent, yielding only to Hawaii and Maui, and nearly of the same extent as Tauai, but is superior even to the largest of them in the proportional quantity of cultivable land.

Its extreme length, from *Koko* on the south-east to Keinna on the north-west, is forty-five miles. A straight line joining these two points, forms an angle of 58° with the meridian, and is more obtuse than the line of direction of the two chief groups of mountains. The largest portion of the island is situate to the north-east of the above line, not much more than a third being to the south-west. The mountain groups are situate one on each side of the line. The extreme breadth of the island is from *Kahuku* on the north, to Laaloa (Barker's Point) on the south, twenty-eight miles. The line joining these two points forms an angle of 26° with the meridian, and of 85° with the extreme longitudinal line. About four-fifths of the island are to the east of the line, and only one-fifth to the west of it.

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[*] Dr Gairdner, who communicates the above Sketch, is author of the excellent treatise, entitled, "Essay on the Natural History, Origin, Composition, and Medicinal Effects of Mineral and Thermal Springs." Before leaving the University of Edinburgh, he had already distinguished himself by great acuteness, sound judgment, and extensive range of knowledge. Meteorology and general physics were with him favourite pursuits;—to geology and zoology he was enthusiastically attached,—and the charms of botany were far from being unknown to him. Fortunately he was too late for the Landers' expedition to Africa; but having resolved to explore some other country, he accepted the appointment of medical resident on the Columbia River. In that remote region, the presence of such a man cannot but prove advantageous to science, and also to the interests of the Hudson's Bay Company, under whose auspices he is placed. The liberal and enlightened members of that body, we doubt not, will find it to be their interest to encourage and give every facility to Dr Gairdner.—EDIT.

The general form is that of an irregular quadrilateral figure, the longest side being directed to the north-east, and extending from *Koko* to *Kahuku* in a straight line thirty-seven miles. The second regards the S.S.W., and extends from *Koko* on the east to *Laaloa* on the west, thirty-four miles in a straight line. The third, which regards the W.S.W., extends from *Laaloa* on the south to Kaena on the north, eighteen and three quarter miles. The fourth and last, regarding the north-west, extends from *Kaena* on the west, to *Kahuku* on the east, eighteen and a-half miles.

I had no opportunity of visiting the eastern portion of the island to the east of a line joining *Kailua* on the north with *Leahi* (Diamond-pt) on the south, except the cursory view from the ship in coasting it on our arrival at the island.

There are two principal chains or groups of mountains. The *easternmost* and most extensive, to which I shall give the name Ronahuanui, from its highest summit extends in a direction nearly parallel with the north-east side of the island. I examined it from *Ronahuanui* on the south-east, to its termination in the sea, near the point of Kahuku. It probably also terminates in the sea to the east at *Koko*, as at sea I saw a ridge extending from the lofty summits down to this point. It may strictly be denominated a chain consisting of a series of denticulated summits, separated from each other by precipitous *paris* or passes. The most elevated part of the chain is confined to a limited portion of its extent in length, about twelve miles from Ronahuanui to the mountains of Kualoa; to the north-west of the latter the summits decrease much and suddenly in altitude lose much of their precipitous and denticulated form, and become more spread out into rounded hills and elevated plains, corresponding to the increased breadth of the island. The general direction of this chain is north 35° west. The average height of the chain from that of a point that I measured on the north-west declivity of Ronahuanui, which seemed to be on a level with the general ridge, extending to the north-west, is 1638 English feet above the level of the sea. But the north-east and south-west declivities are very different in form and extent. The centre of the chain is only from two to three miles distant from the north-east coast, and does not descend to it by a gradual declivity but abruptly, forming one tremendous line of precipices from *Ronahuanui* to *Kualoa*, the steep rocky face being only interrupted by a few projecting buttresses, often proceeding from the chain in a semicircular concentric form, enclosing deep pit-shaped ravines, open towards the sea. At the base of the *pari* or precipice of Anwanu, the soil is elevated 530 feet above the sea. The narrow plain between the precipices and the sea is diversified with hillocks, water-courses, and undulating plains. On the other hand, the central chain is from six to nine miles distant from the sea-shore on the south-west side. The descent on this side is gradual by valleys of greater or less extent, separated from each other by very regular lateral chains or ridges. Few precipices are seen on this side, and none of any magnitude except at the head of the valleys in the circus, which usually terminates them. Hence the mountains have, when viewed from this side, none of that grand and imposing appearance that they have when viewed from the other, and are most accessible on this side. Time did not permit of my ascending the summits of any of the peaks of this chain.

The *westernmost* mass of mountains, to which may be given the name of the mountains of Raala, from that of their highest summit, extends somewhat in the form of a semicircle or amphitheatre, from the point of *Raena* on the north-west, to that of Laaola on the south-east, enclosing and separating from the rest of the island the district of Wainae, extending along the sea-shore between these two points. In general outline, this is much more regular than the chain of Ronahuanui, for although it presents the same needle-shaped peaks and intervening precipices, these rise by a regular gradation from Raena to *Monna Raala,* and descend with nearly equal regularity to *Laaloa. Raala,* a mountain with a table-shaped summit about half a mile in length, extended in the direction of the chain, is not in its middle but about one-third of its length from *Raena,* and two-thirds from Laaloa; it is elevated 3850 feet above the level of the sea, being the highest summit in the island, Ronahuanui perhaps excepted. The total length of this chain is about twenty-three miles.

From its sides are given off very regular lateral chains, which descend gradually from the lofty summits, till they are lost in the plains, enclosing deep and narrow valleys which terminate in the precipitous rocky walls of the central chain. The only means of ascending the central chain is by following the crest of one of these lateral ridges, in consequence of the tremendous precipices which bound the valleys both at their head and sides. This applies more especially to the ridges descending from the summit of *Raala.* On the north-east, the denticulated summits of the central ridge generally correspond to the point of junction of one of the lateral ridges, and the intervening passes to the head of one of the valleys.

The general direction of the highest part of the chain forms nearly the same angle with the meridian as that of *Ronahuanui.* The chain was not examined on the west side, that of Wainae; a view of this side was only obtained at sea, at the distance of from twelve to fifteen miles. The eastern acclivity of the chain is of unequal height, in consequence of the varying elevation of the base from which it rises. The northern portion as far as Raala rises almost immediately from the level of the sea. The base of *Mouna Raala* itself, which is about three miles from the sea, is 312 feet above its level. Five miles of the chain south of *Raala* rise from the plains of Eva, the elevation of which an accident which happened to my barometer prevented me ascertaining, but upon a rough estimate is from 300 to 400 feet above the sea.

The lateral ridges are in some places so steep, that even on them it is impossible to ascend the central ridge, to which all access is then prevented by inaccessible precipices.

The *valleys* of the island are of two kinds, either those descending between two lateral ridges from the central ridge of the chains, and which, from their depth, and the precipitous character of their bounding sides, should be more properly called *ravines ;* and those which furrow the open plains, extending between the mountains and the sea, whose depth is inconsiderable compared with the former, and in which are found the largest spaces of cultivable ground and the principal villages. Of the latter, one of the largest and most fertile is that of Pearl River; and as examples of the former may be cited, those which descend from *Raala* on the side of Waialua. The valley of *Aunanu*, behind Honoruru, is of a mixed character, in its lower part being open, in its upper enclosed between two ridges descending from the peak of *Waolani* on the west, and *Ronahuanui* on the east. This valley possesses this peculiarity, that its upper part forms a level plateau of a circular form, open towards *Honoruru* and the *Pari*, and enclosed on the other sides by the central ridge, and the lateral ridges descending from the above two mountains. This plateau of *Aunanu* thus forms an immense circus, bounded on all sides, except where open, by tremendous precipices, of which a most magnificent view in all its compass may be obtained, by ascending a few hundred feet above the *Pari*, on the ridge of *Ronahuanui*,—a view which embraces not only the amphitheatre on the south, but the extension of the ridge to the west, with the plain of Pali-koolaii, extending at your feet to the ocean, here forming a bay, bounded by the romantic promontories of *Makunana* and Rualoa. There is a third kind of valley, partaking of the character of the ravine, but deprived of every thing except its upper termination, so as to have the form of deep pits enclosed on all sides by precipices generally of great height, as they descend immediately from the central ridge to an inconsiderable level above the sea, except an opening of greater or less width towards the low country. When this opening is very narrow, or even in any case, they resemble very much the interior view of a volcanic crater, opened up by the removal of a portion of its walls. This kind of valley is seen in greatest perfection on the north precipitous declivity of the chain of *Ronahuanui* between the *Pari* of *Anuanu* and *Kualoa*. One just above *Waiahole* is very remarkable for its depth and narrowness, resembling exactly the section of an immense chimney rising from the heart of the mountain, an effect which is heightened by the black colour of the rocks. With the exception of *Mouna Raala*, all the mountain summits in the island belonging to the central ridges of the two chains, are needle-shaped or conical. Raala is table-shaped. I put out of view at present the isolated hills rising out of the plains. The distance between the two parallel lines of direction of the mountain-chains is sixteen miles.

The only communication between the districts of *Rona* in the south and that of *Koolaii* on the north side of the island, is by crossing over the precipice which forms the northern declivity of the chain of *Ronahuanui*. The spots selected for this purpose are the passes or fissures between two adjoining peaks of the chain. Those most frequented are the Pari of *Anuanu*, at the upper extremity of the valley of the same name, elevated 1144 feet above the level of the sea, and one about two to three miles farther west, which, according to the reports of the natives, for it is seldom resorted to by the white residents, is a still more difficult passage than the former, and, when viewed from the low lands of *Koolaii*, appears of much greater elevation.

The *Plains* or *Low Lands* of the island are of two kinds: the belt of low land extending between the mountains and the sea, generally of small breadth, and the elevated plain or plateau of *Eva*, which is the only bond of connection between the eastern and western chains. I am uncertain whether the extensive quadrangular space occupying the north angle of the island, and chiefly comprised in the district of Koolanloa, comes under the denomination of an elevated plain similar to that of Eva, or whether it consists of a tract of hilly country of inconsiderable altitude, forming the northern termination of the chain of *Ronahuanui*. I am inclined, however, to the latter view, from its aspect in crossing the plain of *Eva*.

The low land between the mountains and the sea is either diversified with hills and intersected with water-courses, when its breadth is between two and three miles, as on the north side of the island, between Railua and Rualoa; or it is perfectly flat, forming sandy downs, not more than five to six feet above the sea, extending to the base of the hills, which rise suddenly in tabular cliffs. Its breadth varies, but is never much more than a mile; and, in the vicinity of *Laie* and at *Waimea*, the rocky cliffs approach within a few hundred yards of, or even run into, the sea. This is the character of the low land extending from the mountains of Kualua round the north point of the island, as far as the bay of Waialua.

The plain of *Eva* rises by a gradual acclivity from the low land of Waialua on the north, and of Eva on the south, to the estimated height of 200 to 400 feet above the sea. It is of inconsiderable extent; upon a rough estimate, about forty square miles. It is barren, destitute of wood, but thinly inhabited, and the uniformity of its surface is hardly relieved by the ravines of water courses with which it is frequently intersected, and which render travelling across it tedious and fatiguing.

Not an unimportant feature in the low land of *Eva* are the lagunes at the mouth of the *Pearl* River. These are formed by breaks in the coral reefs, which form nearly a continuous line all round the island, or rather several parallel lines, one behind the other.

There yet remains to be noticed a very remarkable feature in the physiognomy of Oahu, and one of the first to strike the eye of a stranger, from their accumulation at the eastern extremity of the island. These are the isolated hills observed rising either from the maritime low land, or from the midst of the hilly country. In general form they exactly resemble some of the extinct craters of central France, and are doubtless of the same character. They are all open at top, and their outer walls are furrowed by lava streams. They are generally of a brownish colour externally. I counted no less than five in sailing along the east end of the island, including that of *Diamond Point*; *Punchbowl Hill*, just behind the *Honoruru*, is a well marked one. The salt lake of *Aliapaakai*, about four miles to the west of Honoruru, is of the same character, and is elevated only a few feet above the level of the sea, from which it is not more than one-fourth of a mile distant. According to report it is 40–50 fathoms deep, in form nearly oval, and about one-fourth of a mile in its shortest diameter from east to west. Formerly it yielded large quantities of salt, which was dug out in solid masses from the bottom; but this source of wealth has been stopped by its inundation preventing all access to the salt by such means as the islanders possess. The north-east margin of the crateriform hollow is the highest, and is connected with a small tract of hilly country, extending here up to the mountains.

These isolated crateriform hills only represent, on a small scale, and from their being more detached, with all their characters better defined, the larger crateriform peaks of the two chains, which being aggregated together in a linear form, lose at least the outline of two of their sides. Such is the idea I have formed from an attentive consideration of the *paris* at the head of the ravines; they are only the inner walls of the crater, which has poured forth the two ridges, or lava streams bounding the ravine.

The total want of any dome-shaped summits, is a remarkable feature in the mountains of Oahu, and one by which they differ materially from Hawaii, where this form is very common, and it is even reported that you can ride to the very summit of *Mouna Roa.*

In consequence of the peculiar form of the principal mountain valleys of Oahu, there are hardly any rivers, but only mountain streams. The only one which deserves the name, is that which discharges itself into the sea at *Waialua*, on the north-west side of the island. It does not derive its chief supply of water from either of the mountain chains, but from the plain of Eva, and the hilly tract Koolauloa (plain ?) between which it forms a sort of natural boundary. The length of its course is about ten miles.

General Sketch of Formations.

These may be viewed as consisting of two great classes, the *volcanic* and the *coralline.* The first far exceeds the second in quantity, and forms the whole of the interior of the island, as far as it was open to my inspection. The latter is confined to the reefs skirting the sea-shore, or to the cliffs immediately bordering upon it. The former is now on the decrease, from the disintegrating effects of the weather not being counterbalanced by a counteracting formative force. The latter is upon the increase, from the incessant labours of its numerous live inhabitants. The coralline is a recent formation, superimposed upon the previously formed volcanic hills, unless we adopt the hypothesis of part of this limestone mass having been upraised from the bed of the ocean, by the heaving up of the volcanic mass, which is strengthened by the compact and ancient appearance of some of the coral cliffs above *Kahuku*, at the north point of the island.

Basaltic Lava.—The volcanic formation might be viewed as consisting of two parts,—the more compact and basaltiform, and the scoriform; but these, although in the extreme forming very distinct appearances, pass into each other by insensible gradations; the black basaltic compact rocks are generally seen at a small elevation above the level of the sea, as on the sea-shore to the west of Waihea, on the north side of the island, and at the entrance of the valley of *Anuanu.* The more porous and scoriform rocks generally form the higher grounds, such as the lateral ridges descending from the central chains. But this last rule has many noted exceptions, one of the most striking of which is, that, at *Waimea*, on the north-east coast, where scoriæ almost as cellular as pumice are situate nearly at the level of the sea, and under more compact rocks of a porphyritic character, having a dark-blue basis, with imbedded crystals of glassy felspar and olivine, associated in the same hand specimen.

Even the blackest and most compact rocks, when closely examined, are very different from the basalt of the secondary rocks. They have all a certain degree of porosity, however small, and present angular interstices in their mass, giving the idea of their having been formed by agglutination from a semiviscid mass. The following is the series of these rocks that I collected in the order of their similarity to *basalt*, Nos. 13, 1, 35, 23, 12, 48, 49, 11; 40, 33.* The two last form the bond of connection with the next series, the scoriform. The first is undoubtedly the nearest to basalt, both in its external characters and order of superposition, being from a bed on the sea-shore, near Waihea, which runs into the centre of the island. It will be observed, too, that the nearer in the above order these rocks approach No. 13, the darker is their tint, and the freer they are of imbedded minerals. As we recede, the tint becomes more grey, and we have numerous crystals of glassy felspar, of small size, and grains of olivine, giving to the rock a porphyritic character.

Amygdaloidal Lava.—The next series of the volcanic rocks are the amygdaloidal, under which may be comprehended specimens No. 18, 17, 8, 20, 28, 36, 48, 19, 10, 22, 3, 9. The last connects them with the next series, the scoriform; the amygdaloidal cavities are generally empty, seldom full; their inner parietes are often lined, as in No. 28, with a greenish matter. In all these specimens, with the exception of No. 17, the cavities present the idea of having been formed by the extrication of gaseous matter, in a viscid mass, not of cavities which had been once filled with crystalline matter that had subsequently disappeared. In some, the approach to the scoriform character is very close, in others, scoriform, and nearly compact portions are placed in close juxta-position in the same hand specimen, as in No. 8, 9, and 10, which shews upon how weak a foundation is built the determination of the age of these rocks, by their degree of compactness simply. Sometimes a rock will be in one portion of the mass compact and basaltiform, while in others closely adjacent, it will be filled with empty amygdaloidal-shaped cavities.

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* These numbers refer to the specimens sent home with the sketch, and now deposited in the Royal Museum of the University of Edinburgh.

Cellular Lava.—The third series of the volcanic rock are the *cellular* or scoriform, comprising specimens, Nos. 24, 5, 25, 44, 27, 26, 8, 45, 43, 6, 41, 4, 7, 37, 39, 47, 38. The cells vary in number from a few scattered amygdaloidal-shaped cavities, till the whole mass resembles closely in porosity some kinds of pumice. In some the roughness and unyielding texture of the mass, and the size and regular form of the cells, give to the whole a slaggy appearance. In many and in all the more cellular varieties, the cavities are empty; in others they are filled with crystals of olivine, which are frequently undergoing decomposition, leaving the cavities partially empty. Angular crystals of mica and felspar frequently give to the rocks a porphyritic character; these crystals are, however, always of small size. The mica is frequently very abundant, forming, with its shining hexagonal plates, the only imbedded mineral. This is well marked in the slaggy lava from the coast at Waimea. In some of the more cellular varieties, the laminæ of mica become more elongated, and are laid along the interstices of the cells, which they never cross. The felspar is white, and dull in lustre. In the lavas of one of the ridges descending from *Mouna Raala*, it forms a very abundant ingredient, giving to the rock a spotted appearance. This felspar diminishes sometimes so much in the size, whilst the number of its crystals increased, that it forms part of the mass of the rock, which, in such cases, is less cellular, but more compact, rough, and trachytic in its fracture. The most common colour of the basis of these cellular lavas is ash-grey. Occasionally the tint becomes reddish, distinct from that of the matter lining the cavities which has become decomposed. At *Waimea* some of the most slaggy and scoriform lavas that I saw had a brownish-red colour, passing to cochineal red.

Tuffaceous Lava.—The fourth and last series of volcanic rocks that I noticed, are such as may be classed under the denomination of tuffas. These vary in consistence, from the loose and friable reddish tuffa of the pari of Anuanu, to the compact beds alternating with a basaltic rock on the sea-shore near Waihea. These last are full of amygdaloidal cavities, containing radiated zeolites. The specimens comprised in this series are Nos. 2. 15, 16, 14, 50, and 51. They almost invariably form subordinate beds of greater or less extent in some of the preceding kinds of volcanic rocks. One of the largest of these is at the brink of the pari of *Anuanu*, and to this circumstance of geognostic structure is the existence of this pass owing, which forms so remarkable a feature in the physiognomy of the chain Ronahuanui, and which formerly decided the political fate of these islands. In the bed of a stream, at the base of Raala, I observed interposed between dark coloured compact lava a bed of a substance, which, although in consistence much resembling tuffa, yet was very different from it. Some resemblance it had to the *peperino* of Italy. In a basis saffron-yellow, streaked with gallstone-yellow, were imbedded dark-green harder crystals, apparently of augite. The basis of these tuffas was earth, resembling much the earth obtained from the decomposition of lava re-agglutinated together into a firm mud. Its colour varied almost in every locality. In some, wood-brown, wood-brown with a yellowish tinge; in others, tile-red. At the *pari* Anuanu, where it formed so large a mass, it was light orange-red. Such is a brief outline of the leading generalities of the volcanic rocks; let us now sketch, in the same rapid way, the chief characters of the second class of rocks, the

Coralline Formation.—The best situations for judging of the relations of this formation to the volcanic, are on the north-east and north-west shores of the island. The coral cliffs or reefs, when submerged, form a series of terraces elevated one above another, in proceeding inwards to the interior of the island. The line of these ledges, when under water, can be traced by the successive lines of surf that break upon them in ranges behind each other. The space between the last ledge above high water-mark and the first cliffs inland, forms a flat space of greater or less breadth, along which the coast-road lies. This is most generally covered deep with coral sand, which in some places, as between Kahuka and Laie, forms elevated downs along the coast, which conceal the sea from the view of the traveller on the road. The specimens of this series are Nos. 31, 32, 52, 53. One of the best places for seeing a section of these cliffs is just at the north point of the island, above Kahuku; it there forms a precipice elevated at its summit about fifty feet above the level of the sea. The limestone forming these cliffs is of two kinds. One is loose and cellular, of a straw-yellow colour, is easily decomposed by atmospheric agency; when examined with a glass, it appears to be an aggregate of white botryoidal-shaped fragments, and is covered with an ochry looking layer of colouring matter. The other is harder and more compact, affords more resistance to the weather, and projects from the cliff beyond the former; it is of a yellowish-grey colour, with a small granular foliated, partly, and splintery fracture. It is singular that the latter, which appears the older rock of the two, in mineralogical characters, should be superposed to the former; it forms a tabular mass, running along the whole of the upper edge of the cliff, giving to it that prominence and steepness which renders it a conspicuous object for several miles on the approach from Laie, on the south-east. These coral cliffs, for such I would call them on account of the close similarity between the looser variety and the coral reefs, now forming at the water's edge, form extensive tabular cliffs for some distance round the north point of Oahu at a nearly uniform elevation.

The sand forming the extensive plains and downs at the base of these cliffs, is of a dazzling whiteness. After being kept in paper for some time, it has nearly a smoke-grey colour, and consists of variously coloured particles, varying in size from fine dust to that of a millet-seed, made up of fragments of shells, microscopic shells, comminuted limestone, and portions of a dark coloured rock, probably lava.

The *soil* of Oahu is of two kinds, either sand or a deep black earth, arising from the decomposition of lava. Both are so very porous that where there are not abundant facilities of irrigation, the barrenness is extreme. The *taro* fields are almost all in hollows, with the deep lava soil at bottom, and a running stream in the middle. At first sight the lava soil appears to be a fertile black mould, but it is nothing more than an accumulation of round gravelly grains of lava rock.

Stratification.—There were tendencies to this in some craters at the east end of the island, and at the plain of Eva, near Pearl River, at the salt lake; but these I had no opportunity of examining. The only distinct traces that I observed were on the north-east coast, near *Waihea.* I there met with specimens Nos. 13, 17, and 29. It consists of an alternation of beds of dark coloured compact basalt rock, from one to six feet thick, alternating with still thicker beds of grey colour, tuffaceous and amygdaloidal rock, different from any seen in other parts of the island. One of the beds (No. 17,) could not be distinguished from some kinds of greywacke slate in texture and colour of the basis; it contains disseminated numerous white points, which are probably crystals of calcareous spar. No. 14, is not unlike some kinds of wacke. The basis of Nos. 15, 16, 29, is nearly the same as 14, but contains larger amygdaloidal cavities. The whole of this series of rocks is directed N. 55° W. by compass, nearly parallel with the general direction of this side of the island, and the dip to the north-east, rising to the centre of the island. One of the most likely modes of accounting for the formation of these strata, which differ so much from any of the other rocks of the island, is to suppose that they were upraised at the time of the elevation of the central masses of Oahu. Whether the basalt ought to be included in this category, or ought to be viewed as a vein traversing the other beds, and standing in connection with the lavas of the interior, is doubtful. This relation would merit further examination. The only other place where I saw symptoms of stratification, was in the bounding walls of some crater-shaped ravines in the vicinity of Panalau, to the west of the promontory of Kualoa. These seemed to dip regularly outwards on all sides, from the crater, of the position, of which they gave the most indubitable evidence. Some appearances of the same nature were seen in the bounding hills of the salt lake, on the opposite side, and in some of the craters that we passed at sea, at the west extremity of the island.

There can be no doubt that the lateral ridges, descending with such regularity from the central rides of the two chains, are ancient lava streams. This is proved not only from the nature of their component rocks, but also from the form, being narrow and more elevated on their upper part, broader and less elevated as they descend into the plains. One of the finest views of this arrangement is obtained in the streams descending from the north-east side of Mouna Raala, as seen at a distance of five to six miles from the sea-shore at Wailua. The depth of the ravines which separate these lava streams is so great, and their bounding walls, the sides of the lava streams, so steep, that the only way of reaching the central summits is by following one of these lateral ridges. Even when more open, as on the south side of the chain of Ronahuanui, they always terminate in a cul de sac or circus, bounded by tremendous precipices reaching from the bottom of the valley to the ridge of the lava stream, here, near its summit, almost elevated to the crest of the central chain, or even, as at *Ronahuanui*, on the side of the valley of *Anuanu*, to the very highest summits themselves. It is probable that this last arrangement holds also on the north-east side of *Raala*, but the valley is here more elevated at its upper termination than in those which surround the base of *Ronahuanui*, and consequently the bounding precipices, though reaching to the highest summits, are not so lofty. These valleys are directed, as might be expected from the above mode of origin, at right angles to the central ridge, and their walls are straight, without any salient or re-entrant angles.

An interesting question suggests itself. How have the limestone cliffs in the vicinity of Kahuku attained their present position? for their summits are elevated upwards of fifty feet above the highest level reached now by the sea. They must have been under water at the time of their formation, for coral never increases above the surface, the tenants which formed it then dying. Successive elevations of a moderate amount would account very well for the terraced form assumed by the successive reefs rising above each other in regular succession. That the outer reefs become gradually deeper in proceeding out to sea, is evident from the successive increase in the magnitude of the breakers in proceeding outwards, indicating a greater depth of roll, and the outermost is invariably the largest and most magnificent.

On the Number of Comets in the Solar System; on their Light, whether Reflected or Emitted; and on the Comet which will pass its Perihelion in November 1835. By M. Arago.*

1. *On the Number of Comets.*

Although the question as to the number of comets has received a large share of the attention of astronomers, yet satisfactory observations are comparatively so recent, that we cannot attempt to offer more than the statement of certain probabilities on the point.

M. Lalande, in 1773, calculated that there were about three hundred in our system. On the present occasion, I purpose again to go over the ground by which he arrived at this conclusion, always, however, applying his reasoning to the numerical data which have been supplied us by the observations which have been made from the year 1800 up to the year 1830.

In this interval of thirty years, thirty-eight comets have been observed, without taking into account the appearances of the comet of 1200 days, nor that of six years and three quarters. This gives a supply at the rate of four comets for every three years.

If the duration of the comets which we now see was not more than 200 years, we might learn from the historians and chronologists the traces of the previous appearance of every one of them; for since the year 1600, all the celestial phenomena have been noted with sufficient accuracy. And we may here take the liberty of adding, that, whenever it was possible to observe any of these luminaries for a period of some weeks, the ellipticity of their orbits would be sensible, provided the duration of their revolution did not exceed 300 years.

Let us adopt, then, the period of 300 as the *mean term* for the time occupied by a comet in returning to its perihelion. Starting from any given epoch, so long as this period of 300 years has not expired, we should constantly see new comets appear; and this period once elapsed, the same luminaries would again return, though it might be in a different order.

The comets being thus all new ones during the period of 300 years, if every three years were to supply four comets, as has just been said, the 300 years would furnish 400 comets. Such, then, according to this mode of reasoning, would be the number of the comets of the solar system, which would be visible from the earth.

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* The above observations on Comets are taken from the third edition of Arago "Des Cometes en Generale," and the last volume of the Annuaire.

But I shall not stop now to canvass these calculations, that I may immediately address myself to considerations of a much more elevated character, by the help of which Lambert had previously endeavoured, in his ingenious *Lettres Cosmologiques*, to arrive at a solution of the curious problem which forms the subject of this chapter.

On the 31st December 1831, the number of comets whose orbits could be accurately calculated was 137. Let us now examine if, in their movements, these luminaries shew a tendency for any peculiar epochs, or any especial directions.

Epochs of their Passing the Perihelion.—In January there pass 14 comets, February 10, March 8, April 10, May 9, June 11, July 10, August 8, September 15, October 11, November 18, December 13; total 137.

There are evidently fewer comets in the summer than in the winter months; and it could scarcely be otherwise, on account of the shortness of the nights during the months of May, June, July, and August. The long duration of the day properly so called, and also the brightness of the twilight, cannot fail to hide from our view a certain number of these bodies.

Direction of their Movements.—Number of direct comets, 69; number of retrograde, 68; total 137.

If this comparison had been made when the number of comets whose orbits were accurately calculated was only 49, there would then have been 24 direct, and 25 retrograde, in their movements.

Inclinations of their Orbits.—From 0° to 10°, the number of comets is 9; 10° to 20°, 13; 20° to 30°, 10; 30° to 40°, 17; 40° to 50°, 14; 50° to 60°, 23; 60° to 70°, 17; 70° to 80°, 19; 80° to 90°, 15; total 137.

It seems to follow from this table, that the comets are more common in the greater inclinations than in the smaller. Bode had come to the same result, from considering the elements of 72 comets which were known in 1785. At the same time, we have only to glance at the catalogue of this astronomer, to perceive, without resorting to the doctrine of probabilities, that even 137 observations will not authorize us to affirm that there will be always fewer comets near to the ecliptic than at a distance from its plane. In the above table, it may be remarked that there are six comets more between 50° and 60° than between 60° and 70°; whilst the table of Bode gave a difference of four, but in the contrary way. It must be left, therefore, to posterity to decide, whether the primordial physical circumstances, in virtue of which the principal planets are found assembled in the neighbourhood of the elliptical plane, have exercised a different influence on the movements of the comets.

Longitudes of the Ascending Nodes.—From 0° to 30°, the number of nodes is 12; 30° to 60°, 12; 60° to 90°, 20; 90° to 120°, 8; 120° to 150°, 12; 150° to 180°, 13; 180° to 210°, 14; 210° to 240°, 11; 240° to 270°, 10; 270° to 300°, 8; 300° to 330°, 11; 330° to 360°, 6.

It may perhaps be regarded as a circumstance worthy of notice, that those two regions of the ecliptic, to which only eight ascending nodes correspond, are at exactly the distance of a demicircumference from each other; but, at the same time, the space comprised between the 338th and 360th degree is still poorer in its nodes of comets; whilst the region opposite to it does not, in this respect, present any thing particular to our observation; so that probably we are not to recognize in this circumstance any thing more than one of those fortuitous numerical coincidences, which quite disappear so soon as a greater number of observations are collected.

Longitudes of the Perihelions.—From 0° to 30°, number of perihelions, 11; 30° to 60°, 13; 60° to 90°, 12; 90° to 120°, 20; 120° to 150°, 10; 150° to 180°, 8; 180° to 210°, 6; 210° to 240°, 13; 240° to 270°, 18; 270° to 300°, 10; 300° to 330°, 10; 330° to 360°, 6; total 137.

The future will shew whether, as this table would appear to indicate, the extremities of the grand axes of the orbits of comets exist in a much greater number towards the 90th and the 270th degree of the ecliptic, than at any other point; and whether it is at a right angle to each of these regions that we ought, on the contrary, to expect the fewest perihelions. Any conclusion on this subject at present would be premature; because it is clear that 137 orbits cannot supply general results, free from accidental influences.

Distances of Perihelions.—Between the sun and the orbit of Mercury, 30; between the orbits of Mercury and that of Venus, 44; Venus and the Earth, 34; the Earth and Mars, 23; Mars and Jupiter, 6; beyond the orbit of Jupiter, 0.

It seems difficult, after perusing this table, not to consider it as demonstrated, that the perihelion distances are not all equally possible. Nevertheless, when we devote a little more attention to the several conditions of the problem, we shall probably be led to modify the deductions derived from the first glance. And here it will be expedient, in the first place, clearly to recognise the difficulty.

If the perihelions were uniformly distributed throughout the celestial spaces, the number of those which would exist in those concentric spheres, which are nearest to the sun, and having for radii the radii of the orbits of Mercury, of Venus, and of the Earth, would stand to each other in the ratio of the volumes of their spheres; or, to express it in numbers, as the cubes of 3.9, 7.2, and 10, $(3.9)^3$, $(7.2)^3$, and $(10)^3$; that is to say, as 59, 373, and 1000.

Let us now write, under these last cyphers, the number of comets which are known to be included within the spheres of Mercury, Venus, and the Earth, and we shall find that they are 29, 74, and 110.

The first, 29, is very nearly the half of 59, whilst 74 is not quite a fifth of 373; and 110 is not more than between the ninth and tenth part of 1000. The number of observed comets, therefore, does not augment at all proportionably to the volumes of the spaces which include their perihelions.

But before altogether renouncing this law, it will be right to inquire if, for all the regions, more or less distant from the sun, the number of comets which are perceived may not be the same aliquot part of the total number of those stars, the perihelions of which are placed in these same regions. And all that is required to enable us to answer that it is not, is only to put the question in the precise terms which we have employed above.

The comets whose perihelions are placed between the orbits of Mercury and the Sun, ought nearly all to be observed from the Earth: 1st, Because their angular velocity not being very great, a few cloudy days would not be sufficient to transport them from our hemisphere into the opposite one, in which the earth's curve would hide them from our view; and, 2dly, Because near the sun, and swimming, as it were, in its light, these stars, though they possessed a physical constitution the least of all favourable to it, must reflect a sufficient number of rays to be easily perceptible.

The comets which are included between the sphere of Mercury and that of Venus, as seen from the earth, appear to move more rapidly, and are conspicuously less brilliant, than those included in the former list. All other things, therefore, being equal, a smaller proportion of these ought to be seen.

As to those comets whose perihelion distance differs but little from a radius of the earth's orbit, we shall find that, besides being more feebly illuminated than those which traverse, for example, the orbit of Mercury, in a ratio which, expressed in numbers, would exceed that of 16 to 100, it will also appear that, near our globe, their apparent progress is usually extremely rapid, and that, on this account, they will generally be visible only for a few days; and if during that period the sky be at all cloudy, no signal of their passage will be at all observed.

If, then, it should now be inquired why the table last given enumerates so few comets beyond the orbit of Mars, it will be sufficient to remark, in general, that these stars, whatever their perihelion distance may be, cease to be visible from the earth so soon as their course has transported them to a distance from the sun equal to three or four radii of the earth's orbit; and, consequently, those comets whose perihelion is found situated beyond the orbit of Mars, must run their orbit without being perceived from the earth; at all events, when they are not possessed of a size and a density, and consequently a brilliancy, which is altogether extraordinary.

I shall finally remark, more especially for the benefit of those who are astonished at not finding in the table a single comet whose perihelion extends beyond the orbits of Jupiter and Saturn, that the comet of 1759, after its last appearance, sojourned for five whole years in the ellipsis which Saturn maintains, without the slightest trace of it being perceived during this long period. It would require that the brilliancy of a comet should greatly exceed that of any of the class which have been observed for a century and a half, before we could hope to discover it, even with the strongest glasses, whenever its distance from the sun shall have become equal to a radius of Saturn's orbit.

After having thus disposed of the objections which appear to result from the numerical data which are supplied in the table just alluded to, it will be found more natural that, in endeavouring to determine the number of comets which compose a part of our solar system, we should start with the supposition that the perihelions of their orbits are uniformly distributed in space, unless some physical reason can be alleged to establish that it is not so.

The number of comets really known whose perihelion distance is less than the radius of the orbit of Mercury, amounts to 30. This radius, and that of the orbit of Uranus, are in the ratio of 1 to 49. And the volumes of two spheres are to each other as the cubes of their radii. If, therefore, we adopt the hypothesis of the equal distribution of comets in all the regions of our system, and calculate the number of those luminaries whose perihelions are included in a sphere whose radius is the distance of Uranus from the sun, the following proposition would be supplied to us:—As the cube of 3 to the cube of 49, so 30 to the number of comets sought; or thus, $(1)^3 : (49)^3 :: 30 :$ ——. Or, in working out these numbers, as $1 : 117{,}649 :: 30 : 3{,}529{,}470$.

Thus, within the orbit of Uranus, the solar system should be ploughed by more than three million and a half of comets; or, we should rather find the double of that the true number, when we consider that in this calculation the term which represents the number of comets contained within the sphere of Mercury is certainly much too small, and that it ought to be conceded that the light of the day, and our cloudy skies, and a too southerly declination, removes from our sight not fewer than every alternate one of these bodies.

But Lambert, from considerations borrowed from final causes, has rejected the supposition, that the number of comets augments in the direct ratio of the volumes of the spheres which contain their perihelions; and he has finally substituted, in the previous proportions, the surfaces of these same spheres, instead of their volumes.* This proportion then becomes $(1)^2 : (49)^2 :$ 30 : to the number sought; or, as 1 : 2401 : : 30 : 62,030.

According to this hypothesis, the sphere whose centre corresponds with the sun, and its circumference with the distance of Uranus, would include only from 60,000 to 80,000 comets.

2. *On the Light of Comets; and on the means of deciding whether this light emanates from these bodies themselves, or is borrowed from the Sun.*

It has often been matter of astonishment, and not, perhaps, without reason, that such questions as these should still be canvassed in that science which is regarded as of all others the most advanced. Many individuals have found a difficulty in comprehending how those methods, and those instruments which have led to the determination of *the weight* of the planets, should be ineffectual in determining points apparently so much more simple. To this it may be answered, that at first it was necessary to bestow an exclusive attention to the observation of the phases of these bodies, inasmuch as this was the most direct method of ascertaining the point, and because it had succeeded when tried upon Mercury, Venus, and Mars; and also that, in default of this plan, it is true, that so soon as a comet, favourably placed, shall present itself, the phenomena of polarization will decide, at least for an appreciable part of its light, whether it has been derived from the sun or not. But these more simple and direct methods having hitherto failed, I proceed now to state that there is another mode by which the question may be solved, and I the more willingly publish my views, as they may serve to correct the ideas, often incorrect, which even some astronomers seem to have adopted, concerning the manner in which the intensity of the light of these luminaries varies, on account of their distances from the sun and the earth. I remark, that certain measures of intensity, which in themselves should not be of difficult calculation, seem capable of affording, on the first favourable occasion, a definite solution to this curious problem of astronomy. I shall endeavour to present in this place the principles, though somewhat abstruse, upon which this method is founded.

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* Halley's Table of Comets, which was the only one which Lambert could employ at the time of the publication of his *Lettres Cosmologiques*, contained only twenty-one of these stars, viz. six within the sphere of Mercury, and eleven between his sphere and that of Venus. But 6 + 11 : 6 : : 3 : 1 nearly. The surfaces of the spheres of Mercury and Venus being also nearly as 1 to 3, it was possible for Lambert to demonstrate that the laws of surfaces was conformable to observation. Now-a-days, when the Table includes 137 comets, it will be evident that the law can be no longer so verified, for 30 + 44 is not equal to 3 times 30, *i. e.* 90.

Let us then dwell for a little upon *a point without sensible dimensions, and in itself luminous.* From such a point there would emanate, in all directions, particles of light, which would propagate themselves in straight lines. At the distance of a yard these particles will be uniformly distributed upon the surface of a sphere, having its radius a yard long. At the distances of 2, of 3, of . . . , of 100 yards, *the same number of particles*, or, still more accurately, *the same particles themselves*, gradually elongating themselves from their points of departure, will come in contact with spheres of 2, of 3, of . . . , of 100 yards radius. And the surface of these spheres will go on increasing with the radii. It is, moreover, well known that this increase is not in the ratio of the length of the simple radii, but that it augments in the ratio of their squares, so that, at the distances 2, 3, 4, . . . , 100, the surfaces are 4, 9, 16, . . . , 1000, greater than the distance 1. Thus, it may be affirmed, not only that the particles of light will be not nearly so close together, and that they will separate more and more as they depart further from the radiating points, but also, that this scattering will follow the law of the squares of the distance.

And what has now been predicated of the entire sphere must be equally applicable to each of its parts. If at the surface of a sphere of a yard radius, we allow, for example, 10,000 particles upon the extent of a thousandth part of a square yard, there will be, upon an equal extent, the fourth part, or 2500, at the distance 2; the ninth part, or 1111, at the distance 3; the ten-thousandth part, or 1 only, at the distance 100. And admitting, as has been generally done, that the brightness of an object is proportioned to the number of luminous particles which impinge upon it, we arrive at this important optical law, that *the illuminating intensity of a point diminishes when the distances increase, in the ratio of their squares.*

Let us now proceed, from the consideration of a point without sensible dimensions, to that of a luminous surface *that is somewhat extended.*

Every individual point of this surface will evidently act like the isolated point on which we have been dwelling; that is to say, it will emit a light, the diminution of which will follow the progression of the square of the distances. It is only necessary to add, that, in all positions, a screen placed in the course of the rays will receive a quantity which, compared with that which would have arrived from a single point, will be proportional to the numbers of the illuminating particles, or, in other words, to the extent of the luminous surface.

We have just been considering a single point, which emitted upon the thousandth part of a square yard of surface 10,000 particles at the distance of 1 yard; 2500 at 2 yards; 1111 at 3 yards; . . . at . . . ; 1 at 100 yards. Well then, if there exist 1000 similar radiating points at the same distance from our screen, of the size of the thousandth part of a square yard, it will, without doubt, be sufficient to ascertain the brightness of this screen, to multiply by 1000, all the numbers of particles in the above figures. This multiplication will in no degree alter their proportions; for, if the successive terms of a series are the *fourth*, the *ninth*, the . . . , the *ten-thousandth*, of a certain given number, they will still be the fourth, the ninth, the . . . , the ten-thousandth, when these terms, and the number with which they are compared, shall have all become a thousandth times greater.

The illuminating property, therefore, of a *luminous surface* is, on the one hand, proportional to its extent, or to the number of the particles of which it is composed; and, on the other, it varies, like that of an isolated point, in the inverse ratio of the square of the distances.

Will any one now then be greatly astonished, if I affirm, that, in spite of this law, or rather on account of it, a luminous surface should appear *to the eye* to have the same intensity at all imaginable distances, so long as it subtends a sensible angle? A few brief reflections will cause all that at first appears strange in this result speedily to vanish.

When we set about comparing not *illuminating powers*, but *luminous intensities*, it is necessary to choose in the two bodies under our view, *two portions of the same angular extent*—two circular spaces seen under the same angle—under the angle of a minute, for example; and to discover, whilst simultaneously examining them, which of the two seems the most brilliant. Suppose there reach the eye, through openings of the thousandth part of a square yard diameter, rays, coming from two plane surfaces A and B, which are found, at these openings, of equal intensities; then this equality will not be altered if, the surface B being retained in its place, we shall transport the surface A 2 times, 3 times, . . . times, 100 times, further from us, provided that, at all these distances, the corresponding opening shall be completely filled with rays.

In fact, it is true that, in proportion as the surface A is withdrawn, *each* of its points transmits, into the circular opening which the observer employs, a progressively decreasing number of rays; whilst, on the other hand, the extent of the surface which the eye discovers, through the same opening, is so much more extensive; it embraces a number of luminous points so much the more considerable as the increase of distances is the greater. We must now examine if these two contrary results compensate the one for the other.

Every one will comprehend, that the diverging lines proceeding from the eye, and striking the two extremities, of different diameters, of the circular opening through which we look upon the plane A, will embrace upon the plane, rectilinear intervals equal amongst themselves, and the extent of which will be proportional to the distance which separates it from the observer. Thus, at the distances 1, 2, 3, . . . , 100, the real length of the diameters of the circles which we may discover upon the surface A will be in the ratio of the numbers 1, 2, 3, . . ., 100; and geometry teaches us that the surfaces of circles vary in the ratio of the squares of their diameters. The number of the luminous points of the surface, therefore, which we shall perceive through the circular opening at the distances 1, 2, 3, . . ., 100, will be among themselves as 1, 4, 9, . . ., 10,000.

Thus, on the one hand, the intensities of the luminous opening would *augment* as the number of the illuminating points, or as the squares of the distances; but, on the other hand, on account of the divergence of the rays, the quantity of them which the opening embraces *diminishes* for each illuminating point proportionably to the same series of numbers. These two effects, therefore, exactly compensate each other, and therefore also, at all distances the opening should appear equally bright.

A very simple example will fix, without any ambiguity, the true meaning of this important result.

The sun, seen from Uranus, appears to be a very small circle of 100″. Well, then, do you, an observer, standing upon the earth, place between your eye and the sun a metallic plate, which is pierced with a circular opening, whose diameter subtends this same number of seconds, and the portion of the luminous disc which you will thus discover will be equal in size and in *brightness* to the sun as seen from Uranus. Viewed from this planet, the illuminating particles are removed to a distance from the eye of 753 millions of leagues. Observed from the earth, their distance is nineteen times less, or only 39 millions of leagues. The difference is enormous; but it is also to be observed, that, in the first case, all the points of the surface of the sun, without exception, send their light to the eye; whilst, in he experiment made from the earth with the metallic screen, we perceive through the opening only a very small portion of the luminary. I have already demonstrated that the compensation is perfect *.

These premises then being established, let us see how they can serve to decide the question, whether the light of comets is an emitted or a reflected light?

Let us first prove, that, as to the equality of the intensity, the visibility of a comet does not depend, or depends very little, on the angle which it subtends.

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* In this demonstration I have considered only plane surfaces. The law is not less true with regard to curved ones; but I cannot prove it without entering into details which would extend this article much too far.

When, with the help of opaque screens, we reduce the surface of the object-glass of a telescope to a third, a fourth, a tenth, &c., of its original extent, we diminish in the same proportion the number of the rays which go to the formation of the images which this telescope supplies, or, in other words, we diminish their intensity; and when we substitute for the other glass of the telescope, viz. for that small lens, placed near the eye, and which is called the eye-glass, a lens of the same kind, but of higher powers, the magnifying power—the size—is increased. We can thus give to the objects at which we look twice, thrice, four times, ten times, &c., the dimensions in some observations that we do in others.

The object-glass of the telescope being considered as a fixed opening, if by a change of the eye-glass the magnifying power should be increased, the intensity of the image would go on diminishing, since the same quantity of light,—that which the end of the glass admits,—would then be spread over a greater surface. And it may be perceived, that, by proportioning in a suitable manner the portion of the object-glass which the opaque screens leave uncovered, with the change in the eye-glass, it may be so arranged that the enfeebling which results from the enlargement of the image may always be compensated by the admission of a greater number of rays; so that we may gradually give to the images of the moon, of a planet, of a comet, dimensions twice, thrice, four times, ten times greater, than in a first observation, preserving for them throughout all their modifications the same constant intensities.

If we subject to these various proceedings a comet whose diameter equals, we shall say, a minute, and successively enlarge it, without varying its intensity twice, thrice, four times, . . ., ten times, it may be perceived that, as to equality of brightness, the image of the dimensions of a minute may be very easily increased to an image of two, three, four, . . . , ten minutes *.

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* This experiment, and the consequence which follows from it, cannot give rise to any uncertainty, when the natural intensity of the observed comet is such that it can be perceived only with difficulty,—when an additional degree of enfeebling would render it completely invisible.

After this long preamble, a very few words will suffice to shew how, without any observation, of the phases, or any derived from polarization, it is possible to ascertain that comets shine with a borrowed light.

I have, in truth, but recently established (p. 23), that a body *luminous of itself* ought to have, whether *to the eye* or in any given telescope, precisely the same brightness, whatever be the distance at which it is placed in relation to the observer. I have also proved, on the other hand, that the visibility of a body does not depend on the angle which it subtends, at least when that angle does not descend below certain limits. So much being established, it only remains for us experimentally to resolve the following questions: In what manner does a comet disappear? Is this disappearance the consequence of an excessive diminution in the apparent dimensions of the star, arising from a vast increase of its distance from the earth? May it not rather be attributed to a change in its intensity? Well, then, all astronomers will answer that this last cause of disappearing is the true one. The most part of observed comets, and that of 1680 in particular, have disappeared by a gradual enfeebling of their light. They have become, so to speak, extinguished. The evening of the day in which we cease to be able to observe them, they still subtend angles which are very considerable. But this mode of disappearance, I have proved in detail, is irreconcileable with the existence of an inherent light, and comets therefore borrow their light from the sun.

In the various experiments which have led the way to this conclusion, we have admitted that, during its variations of distance, the luminous body under our observation did not change in its physical constitution; but, it is true that comets do not answer to this description. There is here, then, a substantial objection, and some short reflections will therefore be required.

Till lately, it was very generally believed that the nebulous matter of a comet gradually *was condensed,* in proportion as in its elliptical course it was removed from the sun. This condensation could not fail to produce in the star a brightness *superior* to that which without this change it could have maintained.

Observation has shewn us that this star becomes more and more faint, even in the alternative of a theory, based upon the hypothesis that an unchanging constitution would indicate a constant light. The real increase of intensity which would have resulted from the supposed condensation of the nebulous matter, was calculated, therefore, to exhibit more strikingly the disagreement between the calculation and the experiment. It ought to add strength to the conclusion to which this disparagement has conducted us; and so, in our argument, we might legitimately have abstracted the alleged condensation which the nebulosity of the comet was supposed to undergo. Now-a-days, however, on the contrary, it is proved that, instead of contracting, the nebulosity dilates, as it withdraws from the sun. I would not venture now, as I did formerly in my public lectures, to conclude without more ado, that because the light of comets became gradually more faint, therefore their light is a reflected light. It is necessary, in addition, to take into account the scattering which the nebulous matter undergoes. It is necessary to demonstrate, that the real diminution of intensity which thence results is not sufficient to explain how, sooner or later, the most brilliant comets disappear. But this appears neither difficult nor complicated, as the reader shall judge.

Up to the present time, the most brilliant comets have ceased to be visible from the earth, as soon as, in their progress round the sun, they have removed themselves from this luminary to a distance equal to a radius of the orbit of Jupiter; that is to say, to five times the radius of the almost circular curve which the earth annually describes. Let us now then consider a comet which, like that of 1680, had its perihelion within the orbit of Venus. According to the calculations of M. Valz, the real diameter of its nebulosity would augment with its distances from the sun, according to the following progression:—At the distance of Venus, 10; the Earth, 29; Mars, 76; Ceres, 173; Jupiter, 278.

This progression of diameters differs but little from the following:—1; 3; 8; 17; 28.

The quantity of nebulous matter which, at the distance of Venus, occupies a spherical volume of a diameter equal to 1, will therefore be expanded into volumes of the same form 3, 8, 17, 28 times larger at the distance of the Earth, Mars, Ceres, and Jupiter.

These spheres, diaphanous on account of their extreme length, exhibit themselves in simple circles. It is in the apparent surface of these circles that the same quantity of nebulous particles appear successively sprinkled with greater or less uniformity. The luminous intensity of the nebulosity will evidently vary in the inverse ratio of its density,—will follow the law of the surface of circles; in other words, that of the squares of their diameters, or the squares of the numbers 1, 3, 8, 17, 28.

I have already proved, that a comet which had *inherent light* would not experience, at any distance in which it could be observed, any other variations of density than those of which we have already specified both the cause and the law. We have now, therefore, in addition, only experimentally to examine if these variations are sufficient to render the most brilliant comets invisible, as soon as they have reached the orbit of Jupiter. The following is the method in which the experiment should be undertaken.

A telescope should be selected which had a large opening, and a feeble magnifying power, and with which the comet could be observed during the whole duration of its appearance. So much being ready, the day, for example, in which the star shall be found distant from the sun, a space equal to a radius of the orbit of Venus, it should be examined, first, as a point of comparison, with the most feeble magnifier, and then with magnifiers 3, 8, 17, 28 times stronger. During these experiments, the same quantity of light, that, namely, which the unvarying extent of the object-glass would admit of,—that, in short, which the circular image of the comet depicted in the first experiment, would be found successively spread out upon circles 3 times, 8, 17, 28 times, larger diameters than in the first of the observations. But is it not evident, that the diminutions of intensity which these *artificial* dilatations will induce upon the luminous matter of the comet, will be respectively equal to those which result from the corresponding *natural* dilatations, which the star undergoes in removing from the sun? or, in other terms, that the simple changes in the eye-glass cause the comet to pass, so to speak, in a few seconds, from the distance of Venus to that of the Earth, of Mars, Ceres, and Jupiter? If this, then, be true, and seeing the comet with our telescope supplied with its weakest magnifying powers when it crosses the orbit of Venus, we subsequently examine it in succession with the help of a magnifying power 3 times, 8, 17, 28 times stronger; then, if it be always seen, it ought to appear the same as when seen with the first and feeblest magnifying power, at the epochs in which its proper movement shall have transported it to distances from the sun, equal to the radii of the orbits of the Earth, of Mars, Ceres, and Jupiter; and if it be not seen, for example, when it has reached the orbit of Jupiter, it is not only because it has been subjected to the enfeebling which might result from the scattering of the matter of which it is formed;—it is because it does not act as a body which has an inherent light; it is because it borrows its brightness from the sun!

It is true that all comets are not equally eligible for these kinds of experiments. We ought to select in preference those comets without apparent nucleus, and without a tail, because they appear to be less subject than others to sudden and irregular changes of figure, and because that, during the act of the singular dilatation they undergo in removing from the sun, and of which Mr Valz has given the law, it is probable that all the parts, from the centre to the circumference, undergo analogous changes. Without this condition, the natural dilatation of the nebulosity cannot be assimilated to that which we obtain artificially in the foregoing proof by means of the eye-glass of the telescope. The importance of this remark will be perceived, when I notice that, in the comet of 1770, the nucleus and the nebulosity properly so called, were very far indeed from experiencing proportional changes *.

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* In proof of this assertion, I subjoin the measurements which Messier has given both for the nucleus and nebulosity of the comet 1770:—

On the 17th June 1770, the nucleus measured	0′.22″,	the nebulosity	5′.23″
... 22d ...	...... 0.33	...	18.0
... 23d ...	...... 1.15	...	27.0
... 29th ...	...... 1.22	...	54.0
... 2d July ...	...... 1.26	...	123.0
... 3d August ...	...... 0.64	...	15.0
... 12th ...	...... 0.43	...	3.36

The method which I have now described so much at length, is exposed, I believe, to only one kind of objection; it may be imagined that the matter of which the comet is composed has no inherent light of its own, but that it acquires it when under the action of the solar rays.

This hypothesis, in reality, would be nothing more than the reproduction of that system which Euler has developed in his "Letters to a German Princess;" and, according to which, the light which enables us to perceive certain bodies, such as paper, porcelain, &c. is not composed of rays which are truly reflected, but rather of a particular kind of light, which these bodies engender by being subjected to vibrations under the action of the solar rays. This, it will be perceived, is a difficulty of pure theory, and which might be as much applied to the light of the moon, of the planets and their satellites, as to that of comets. But the sole object which I proposed to myself in this paper, was to discover a method which would enable us to decide whether these bodies were to be ranged, as it regarded their luminous properties, in the same category as our own globe, or as Mars and Jupiter, &c. That other question, whether the light which enables us to perceive coloured bodies is reflected, as Newton supposed, at the surface of very fine material plates, or proceeds from vibrations communicated to the air by the constituent particles of bodies; this question, I say, is of a very different kind and extent, and this is not now the place to discuss it.

3. *On the Comet which will pass its Perihelion in November* 1835.

In the *Annuaire* of 1832 I published a long notice, in which there is an elementary explanation of all that astronomy at the present day possesses of precise and mathematical information on the motion and nature of comets. To that treatise I beg to refer the reader. In the present note I shall content myself with mentioning some modifications made on the former results, pointing out the principal circumstances which may render this sixth appearance of the same comet useful to science, and indicating the constellations in which we are to seek it.

After having determined, by the assistance of very laborious calculations, the derangements or perturbations which the comet at present expected, and known by the name of Halley's comet, must encounter during its progress, from the united attractions of Jupiter, Saturn, Uranus, and the Earth, Messrs Damoiseau and De Pontécoulant fixed, the one the 4th, the other the 7th November, for the time of the comet passing its perihelion, that is, the point of its orbit which is nearest the sun. Since these first researches, astronomers have determined, that the mass of Jupiter, which had been considered as equal to the 1070th part of that of the sun, is equal to the 1054th part. By adapting this new calculation, and by taking more completely into account the action of the earth, M. de Pontécoulant has definitely fixed the passage of the perihelion for the 13th instead of the 7th November.

At the moment of this passage, the distance of the comet from the sun will not exceed six-tenths of the distance of the sun from the earth. At the other extremity of the great axis of the orbit, in thirty-nine years from this time, the distance of the two heavenly bodies will on the contrary be immense. Calculation gives more than thirty-five times the radius of the terrestrial orbit, that is, more than thirty-five times the distance of the earth from the sun.

The result of the calculation of the passage of its perihelion by Halley's comet in 1835, compared with the result of observation, will inform us if this comet, like the small and feeble one which returns at short intervals, is sensibly deranged during its progress by the resistance of the ether. This comparison, in its turn, will furnish us with some notions on the physical constitution of the comet which we expect, for a given resisting medium exercises more or less effect according to the volume and density of the body which traverses it.

Is the ether in repose, or does it move round the sun, from west to east, in the manner of the planets? In the last case, its effect on the comet, which returns at short intervals, which itself moves from west to east, will be different from that which it will exercise on the comet of Halley, whose motion, on the contrary, is directed from east to west. The science of celestial movements, and that of cosmogony, are equally interested in the solution of the problem to which I allude.

We do not yet know with certainty if comets are of themselves luminous, or if they borrow from the sun all the light with which they shine. The investigation of their phases seemed to be the only means of resolving the question, but hitherto it has entirely failed. Comparative measures of the intensity of light, photometric measures, may lead us to the result in a manner not less incontestible. This kind of observation will doubtless excite the attention of astronomers, during the actual appearance of Halley's comet. The second edition of the Annuaire of 1832 proves to amateurs of the science, that they even, with very feeble instruments, may usefully assist in this curious research.

In the year 1305, Halley's comet had extraordinary brightness; in 1456 it dragged after it a tail which embraced two thirds of the space between the horizon and the zenith; in 1682, though notably less brilliant than in 1305 or 1456, it was classed among the brilliant comets, and its tail was still 30′; in 1759, its appearance would certainly have occupied the attention of none but astronomers, if it had not been the first comet announced a long time previously. These facts seem to prove that comets are diminishing in brightness, and we might be tempted to search for the physical cause in the matter which near the perihelion separates itself from the nebulosity to form the tail, and which the comet seems to disseminate in space. M. Olbers, certainly one of the most competent judges in a subject of this kind, does not regard the gradual enfeebling of comets as proved; he believes that the diminution observed in that of Halley from 1305 to 1456, from 1456 to 1682, and from 1682 to 1759, is only apparent; that it can be explained by the very peculiar relative positions of the sun, the comet, and the earth; and finally, he cites in support of his opinion, the appearance of 1607, intermediate between those whose dates I have mentioned, adducing which for the positions analogous to those of 1759, the comet, according to Kepler, presented nothing remarkable in its intensity. In this state of the problem every one must perceive the interest which must be attached to the circumstances attending the appearance of the comet next November.

It will be on August 20. 1835, near ξ of Taurus; August 28, between Gemini; September 21, in the Coachman; October 3, in the Lynx; October 6, in the Great Bear; October 11, the same; October 12, in Arctophylax; October 13, in the Crown; October 15, between Hercules and Serpentarius; October 19, in Ophiucus; October 31, the same; November 16, near η of Ophiucus; December 26, in the Scorpion, near Antares.

It would be difficult to say with certainty on what day the comet will become visible. The state of the heaven, the power of the instruments, the goodness of the power of sight of the observer, united to physical causes, already mentioned, variations of intensity, render all attempts to solve the problem entirely illusory. M. Olbers believes that at its maximum of lustre, the expected comet, far from surpassing, as has been pretended, that of 1811, will not equal the third comet of 1825, which the public allowed to pass without paying any attention to it.

It is towards the middle of October, that the comet of Halley, during its next appearance, will be nearest the Earth. We may add that its distance will never be less than eight millions of leagues, of twenty-five to the degree. Thus those who have not been entirely reassured by the numerous pleadings published lately in favour of comets, cannot in the present case have any plausible cause of uneasiness.

On the periods of the Deluges of Deucalion, Ogyges, and Noah, according to the various calculations of time of different Nations. By Dr Von Schubert.

The history of nations and countries speaks distinctly of several partial floods and deluges which seem to have been produced by lakes at a high elevation bursting through their barriers. The flood of Deucalion, and also that of Ogyges, have been considered as having owed their origin to the outbreak of highly situated masses of water (as, for example, of the Thessalian lake), and they are supposed to have taken place several centuries after the great Noachian Deluge. A more accurate comparison of the various chronological dates, shews that the ancient calculators of time understood no other than the flood of Noah by the floods of Ogyges and Deucalion. This was indicated by the statement of Censorinus, who, in his 21st chapter, fixes the period of the Ogygian Deluge 1600 years before the commencement of the calculation by Olympiads, or in the 25th century before Christ; consequently, at the same time at which, according to Frank and Gatterer, the Noachian Deluge happened. In order to understand the cause of the difference of the other statements of antiquity, in regard to the period of the deluges of Ogyges and Deucalion, which deviate so much from the one we have now given, it is necessary to refer to the various statements as to the time of the Noachian flood itself. The calculation of time in the original passage of the Bible fixes the Deluge in the year 1656, and the Greek translation, in the year 2242 after the creation. These two apparently widely different statements agree more nearly than we should at first sight believe. For the Septuagint employed a mode of calculating time according to years of 272 days, consequently to years consisting of ten months; a natural period which, from its affording an approximation to an adjustment of the periods indicated by the sun and moon, and from its utility in calculating more rapidly eclipses, seems to have been often employed in ancient times in conjunction with the year as calculated by the sun. 2242 years of 10 months would correspond nearly to 1656 solar years.

If we reckon upwards from the point of commencement of our present era, the period of the flood was, according to Gatterer and Frank, 2525 years before Christ; according to Petau, 2328 years before Christ. According to an old chronological system of the Indians, mentioned by Baldäus, and which reckons exactly 100 years more than the present prevailing system in India, the period of the Yudhistira or of Noah should be fixed at 3200 years before Christ, consequently, according to Frank and Gatterer, in the last ten years of the tenth century after the creation. And this system, according to a mode of calculation, which had for its foundation the above-mentioned year of ten months, was that employed in the Samaritan translation of the Pentateuch, when it mentions that the period from the creation to the flood was 1307 years: For 1307 years of ten months are equal to 974 solar, or 1000 lunar, years.

But the period of the Yudhistira, or the beginning of the

Kali-yug, appears, according to the mythology of the Indians, also as a period of the creation, as the beginning of the existing human race. If, conformably with the old Oriental opinion, and as, according to the Hebrew original text in regard to the period from Adam to Noah, 1656 years are reckoned from the beginning of the Kali-yug as from the creation, then the time of the great flood would be fixed in the middle of the sixteenth century before Christ, the period assigned to the Deucalion flood by old chronologists; while, by several determinations of the time, a similar period between the creation and the deluge seems fixed for the Ogygian flood, but in the sense in which that period was employed in the Samaritan translation of the Pentateuch.

Note respecting the application of the Compressibility of Water to Practical Purposes. By JAMES D. FORBES, Esq. F. R. SS. L. & E., Vice-Pres. Soc. Arts, and Professor of Natural Philosophy in the University of Edinburgh *.

ONLY two methods have been applied with much success to the *precise* determination of pressures communicated in all directions; the one, by observing the volume of air inclosed in a tube, as in the common manometer; the other, by the actual measurement of the height of an equi-ponderant column of fluid such as mercury. Each of these methods is subject to grave practical inconvenience: In the case of the manometer, from the immense disproportion of the division of the scale for great variations of pressure, and, in the other, from the extremely cumbrous and unmanageable apparatus which it requires when the pressures are considerable. Both these methods were resorted to by the Commission of the Institute of France, appointed to ascertain the relation of the temperature and pressure of steam, the pressure being ascertained by the volume of air in a manometer, previously graduated experimentally by comparison with the pressure of a column of mercury.

The idea of substituting a manometer constructed of water instead of air, occurred to me a considerable time ago, when applied to by a friend to suggest a form of guage for measuring the pressure of condensed gas intended to be used for a furnace. I had recently been making experiments with the very convenient compression apparatus of Oersted, in which the changes of volume of water and air are exhibited at once, under any pressure, that of the water being sensibly uniform for equal increments of pressure, whilst that of air rapidly diminishes.

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* Read to the Society of Arts on 22d April 1835.

It is the very trifling compressibility of water (or any other liquid) which gives the value to this application, and which seems to have been practically overlooked.

The reason is obvious. The changes of volume produced by a pressure of only one or two atmospheres, in the case of air, are quantities very large in proportion to the primitive volume, so that, in the consideration of an additional change, we are obliged to take into account not merely the effect upon the primitive volume, but upon the volume affected by the first unit of pressure. In other words, we are not at liberty to neglect quantities of the second order, which we may safely do in the case of any known liquid. In the case of water, for instance, the variation of volume for one atmosphere does not exceed $\frac{1}{20,000}$ of the whole; so that the variation of the variation is necessarily insignificant. All that we know of the constitution of liquids, would lead us to infer, that such would be the case, and upon this circumstance depends the *linearity* of the expression, which connects the volume of a liquid such as water, and the pressure to which it is subjected.

Within ordinary practical limits, we may confidently anticipate the sensible proportionality of pressure and change of volume; and this is fully borne out by a comparison of the best experiments on the compressibility of water made within great and within narrow limits.

I did not hesitate, therefore, to recommend the trial of a manometer of water instead of air, for measuring the elasticity of gas up to 40 atmospheres of pressure.

The construction of such an instrument being almost like that of the common thermometer, is incomparably simpler than that of the other instruments above mentioned; and almost the only practical difficulty is common to all these, namely, the accurate determination of the temperature of the fluid employed.

It may be proper to remark, that Professor Oersted's instrument for indicating the compressibility of water, consists merely of a very sensible thermometer, constructed of water, and having the end of the tube left open. The tube being capillary, a short column of mercury rests on the surface of the water, indicating its volume at any moment; and the whole is immersed in water contained in a strong vessel, to which pressure is any how communicated, so that the thermometer-shaped vessel of glass being equally pressed within and without (the neck being open), is unaffected by pressure, and the true change is perceived in the volume of water which it contains.

The applications of this form of instrument are very numerous; we may take as examples,

1. The determination of the tension of gas or air in a compressed magazine, as I have just suggested.

2. The measure of elasticity of high-pressure steam.

3. The determination of the degree of compression under which bodies change their state, when such experiments can be performed in glass vessels, as in the case of the condensation of the gases into liquids, the pressures as stated by different authors varying extremely, and being confessedly imperfect approximations.

4. The ready determination, by inspection, of the pressure per square inch exerted by Bramah's press at any instant.

Nothing could be easier than to convert the instrument as above described into a self-registering one, by simply inserting an index of glass, which may be drawn back by the little mercurial column, just as in Six's thermometer. We should thus be enabled to determine the operation of causes by their nature concealed from direct view; as,

5. The force exerted by water in the act of freezing, in a manner much more direct and satisfactory than that of the Florentine Academicians, because it would not be necessary to cause the recipient to burst, the maximum expansive force being indicated by the register

6. The force of fired gunpowder; and even of dead pressure and of percussion in a variety of cases.

7. The depth of the ocean by the measure of the pressing

column, the instrument being attached to the sounding-lead. I have been informed that the ingenious Mr Perkins proposed this application of the compressibility of water which naturally arose from his method of ascertaining the *fact* of compression by using the pressure of the ocean, though no notice of this is taken in his paper in the Philosophical Transactions. The Piezometer there described was, like Oersted's instrument, intended for measuring *compressibility*, not *pressure*.

In these cases, a Register Thermometer would need to accompany the self-registering instrument. Probably no considerable error is to be feared from abrupt changes of volume to which the water might be subjected, for the coincidence of the velocity of sound in water, theoretically deduced from its modulus of elasticity, and experimentally by M. Colladon, seem to prove that little or no heat is developed during its compression.

The accompanying thermometer would, of course, require to be itself protected from the disturbing influence of pressure.

The extensibility of the glass vessel containing the water under pressure, might be applied to give an independent confirmation of the first result; and elegant practical constructions might be pointed out by which these separate results might be obtained, and also the effect of temperature eliminated; but with these I forbear at present to trouble the Society.

Upon the Egyptian Hieroglyphical Writing.[*] By M. Arago.[†]

The word Hieroglyphic, whether regarded metaphorically, or in its natural acceptation, transports us to a region which has already been the theatre of many and most animated discussions. For a moment I hesitated to encounter those passions which the subject has excited. I was tempted to conclude, that the secretary of an Academy which is exclusively devoted to the exact sciences, might really, without any impropriety, hand over this philological discussion to judges who were more competent to the task. I moreover avow, that I could not but entertain the apprehension, that in many important particulars I should not be of the same mind with the illustrious individual, the analysis of whose labours has hitherto afforded me so much delight, without eliciting a single word of criticism from my pen. But these scruples have all vanished, upon reflecting that the interpreting of these Egyptian hieroglyphics is one of the most brilliant discoveries of our age;—that it was Dr Young himself who introduced my name into the discussion,—and, finally, that to inquire whether France may pretend to this new wreath of glory, is only accomplishing the task which at this moment I am fulfilling—the enacting the part of a good citizen. I acutely feel that I am treading upon delicate ground, and I know that in the citizen of the world there is much to admire; but in what terms would be proper to designate him, if, when every neighbouring country is enumerating with satisfaction the discoveries of her sons, I was not permitted, in the bosom of our own,—amidst associates whose modesty I must not wound,—to enter upon the proof that France has not degenerated, and that every year she contributes her full contingent to the vast stores of human knowledge.

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[*] The above is part of the *Historical Eloge* on Dr Thomas Young, which was read to *L'Academie des Sciences*, in the month of November 1832.

[†] It was thought by some, that at the time when the Chamber of Deputies was called to decide upon the national acquisition of the manuscripts of M. Champollion, and upon the situation of his widow, it would be important for the members of that body to have before them a simple and concise analysis of the great discovery of the phonetic alphabet of the ancient Egyptians, and the interesting discussion which has arisen out of it. M. Arago, who, in drawing up the Historical Eloge of the illustrious Dr Young for the *Academie des Sciences*, was necessarily led to discuss this question, has kindly yielded to the request of the friends of the late M. Champollion, and has supplied them from this production, which has not yet been published, the whole of the part which relates to hieroglyphics. In thus printing it for circulation among the members of the two Chambers, the friends of Champollion cannot entertain a doubt that it will

I take up, then, the question of Egyptian writing, and enter upon it free from all prepossessions, and with the firm resolution to be just; with a sincere desire to reconcile the rival pretensions of two illustrious individuals, whose premature death has been to Europe the subject of sincere regret. Be it also observed, that in this discussion concerning hieroglyphics I shall not transgress the limits which are assigned me; happy if the assembly which now hears me, and whose indulgence I crave,

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accomplish the object they propose, viz. the making manifest his claim to originality in a discovery, of which the incalculable historical importance is sufficiently attested by the numerous efforts which our transmarine neighbours are making, to claim it for themselves.

shall recognize that I have been able to avoid all prejudice on the subject.

Mankind have conceived two systems of writing, which are completely distinct. The one is in use amongst the Chinese, which is the hieroglyphical system; and the other, now employed by every other people, bears the name of the Alphabetic or Phonetic system.

The Chinese have no letters properly so called. The characters which they employ in writing, and which are true hieroglyphics, do not represent sounds nor elementary articulations, but ideas. Thus a *house* is expressed by means of a particular and special character which never changes, even although all the Chinese might have designated their houses in the spoken tongue, by a word totally different from that which they use at the present time! Let it likewise be recognised that our common numbers, also, are all hieroglyphic. The idea of unity added eight times to itself is every where expressed in France, and England, and Spain, &c. by the help of two small circles placed vertically on each other, and touching at a single point; but, in reading this ideagraphic sign, the French pronounce *huit*; the English, *eight*; the Spaniards, *ocho*. It is likewise known that it is the same with all compound numbers. And we may here remark in passing, that, if the Chinese ideagraphic symbols were generally adopted in the manner that the Arabic ciphers are, every man might read in his own tongue all the works he could meet with, without having occasion to know a single word of the spoken language of the writer.

But it is very different with alphabetic writing:—

> That man who taught the truly wond'rous art
> Our speech to paint, and to our sight to speak,

having made the grand initial observation, that all the words of the richest spoken language are composed of a very limited number of sounds or elementary articulations, invented symbols or letters to the number of twenty-four or thirty, for their representation. With the help of these signs differently combined, he could write every language he heard spoken, even when he knew nothing of its meaning.

The Chinese or hieroglyphic mode of writing appears to be the infant state of the art. It is not true, however, as was formerly supposed, that even in China itself, the long life of a studious mandarin must be spent in learning to read. Rémusat, whose name I cannot utter without recognising the severest loss that literature for a long time has sustained, has established both by his own experience, and by the scholars he annually reared by his side, that the Chinese might be learned like any other language. Nor is it, as at first view it might be thought, that the hieroglyphic characters can represent only very simple ideas; a few pages of the romance *Yu-Kiao-li*, or of the *Two Cousins,* will be sufficient to establish that the subtlest and most exquisite abstractions may be embodied in the Chinese writing. The main fault of this kind of writing appears to be its inability, in any way, to express new names. Thus, although a person at Canton might be able to write word to Peking, that, on the 14th day of June 1800, a most memorable battle saved France from great danger; yet he would not know how, in characters purely hieroglyphic, to apprize his correspondent that the plain on which this glorious event happened, was near the village of *Marengo*, and that the victorious general bore the name of *Bonaparte*. But a people among whom the communication of proper names from place to place can be effected only by special messengers, would scarcely have taken the first steps towards civilization, and we are not therefore surprised to learn that this is not the method with the Chinese. It is true the hieroglyphic character forms the great mass of their writing, but sometimes, and especially when they wish to write a proper name, they strip these characters of their hieroglyphic signification, and cause them to express sounds and articulations, in short, they make them true letters

These preliminary observations are far from being irrelevant. The questions of priority, which the Egyptian modes of writing have originated, may now be easily explained and comprehended. We shall find in the hieroglyphics of the ancient subjects of the Pharaohs, all the methods which the Chinese employ at the present time.

Many passages of Herodotus, Diodorus Siculus, and Clement of Alexandria, have informed us, that the Egyptians employed two or three different kinds of writing; and that, at least, in one of them, symbolical characters, representative of ideas, were much used. Horapollon has even preserved the meaning of a certain number of these characters: thus it is known that the *hawk* represents the *soul*; the *ibis*, the *heart*; the *pigeon*, which will appear sufficiently strange, a *passionate man*; a *flute*, a *madman*; the number *sixteen*, a *voluptuary*; a *frog*, an *imprudent man*; the *ant*, *wisdom*; a *running knot*, *love*; &c. &c.

The symbols thus preserved by Horapollon supply but a very small proportion of the eight or nine hundred characters which have been observed on monumental inscriptions. The moderns, and Kircher among the rest, have endeavoured to augment the number of these symbolical characters; but their efforts were useless, except in demonstrating how far the best informed men will go astray, when, in search of facts, they without restraint abandon themselves to the flights of fancy. From the absence of sufficient data, the interpretation of the Egyptian writings had long appeared to all well informed men a problem wholly insoluble; when, in 1799, an ingenious officer, M. Boussard, discovered in the trenches he was clearing near Rosetta, a large stone covered with three sets of characters, entirely distinct. One of these sets was Greek. This, notwithstanding considerable mutilations, expressed that the authors of the monument had ordered that *the same inscription* should be written on the stone in three kinds of characters, viz. in the sacred or Egyptian hieroglyphic character; in the local or common characters, and in Greek letters: thus, by an unlooked for piece of good fortune, philologists found themselves in possession of a Greek text, having in connexion its *translation* into the Egyptian language; or, at least, a copy of it, in the two kinds of character which were anciently employed on the banks of the Nile.

The stone of Rosetta, which has since become so famous, and with which M. Boussard paid fealty to the Institute of Cairo, had been conveyed to that learned body, at the time that the French army evacuated Egypt. It is now in the British Museum, where it stands, says Young, a trophy to British valour! But, valour apart, the celebrated physician might have added, without excess of partiality, that this invaluable two-tongued monument, bore likewise some testimony to those enlightened views which had presided over all the details of this memorable Egyptian expedition, and also to the indefatigable zeal of that illustrious scientific corps, whose labours, frequently executed at the cannon's mouth, have added so much glory to their country. In fact, so strongly were they impressed with the importance of the inscription of Rosetta, that, lest the precious treasure should be abandoned to the perils of a sea voyage, they from the first, enthusiastically endeavoured, so to speak, to reproduce it, now by simple drawings, now by correct impressions, by obtaining copperplate engravings, and finally by moulds, in Paris-plaster and in sulphur. I must also add, that antiquarians all over the world became first acquainted with the pillar of Rosetta from the designs of the French savants.

M. Silvestre de Lacy, one of the most illustrious members of the Institute, was the first to appear, in the year 1802, in that interesting field which this two-tongued inscription opened up to the investigation of philologists. He confined his attention solely to the Egyptian text in the popular character. In it he discovered certain groupes which represented different proper names, and also that they possessed a phonetic character. Thus it was established, at least in one of the two inscriptions, that the Egyptians employed the symbols of sounds, or true letters. This important result was determined beyond contradiction, after the celebrated Swede, M. Akerblad, re-investigating the labours of our countryman, had assigned, with a probability bordering upon certainty, the individual phonetic value of several of the characters employed in the transcription of the proper names, which the Greek text had made known.

Up to this period that side of the inscription which was purely hieroglyphic, or supposed to be such, had remained untouched. No one had ever dared to undertake to decipher it.

It was here that we discover Dr Young declaring, at first, as by a kind of inspiration, that, in the multitude of the symbols which were sculptured upon the stone, and represented, whether by entire animals, or by beings of fantastic shape, or by the instruments or the products of art, or by geometrical forms, those of the symbols which were found in certain elliptical frames corresponded to the proper names of the Greek inscription: he specified particularly the name of Ptolemy, which was the only one which remained uninjured in the hieroglyphical inscription. Immediately after this, Dr Young asserted that all the symbols included in the frame or cartouche, did not represent ideas, but sounds; finally, he endeavoured, by a minute and very delicate analysis, to assign an individual hieroglyphic character to each of the sounds the ear perceives in the name Ptolemy on the pillar of Rosetta, and also in that of Bernice, which he found upon another inscription.

Such then were, if I do not deceive myself, in the researches of Young into the Egyptian systems of writing, the three grand points. No one, we have said above, had previously discovered them, or, at all events, had pointed them out previous to the English physician. This opinion, though generally admitted, I regard doubtful. In truth, it is certain, that as early as the year 1766, M. de Guignes, in a printed memoir, had pointed out that the cartouches of the Egyptian inscriptions all enclosed proper names. Every one may also see, in the same work, the arguments which this learned Orientalist employed to establish the opinion which he had embraced, regarding the constant phonetic character of the Egyptian hieroglyphics. Young, therefore, has the priority only upon a single point; being the first who attempted to decompose into letters the encircled groups, that he might give a phonetic value to the hieroglyphics in the Rosetta pillar composing the name of Ptolemy.

In this investigation, as may be supposed, Dr Young furnished new proofs of his extraordinary penetration; but, misled by a false system, his endeavours were not crowned with entire success. Thus, sometimes he attributed to the hieroglyphic characters a value which was simply alphabetic, or of a single letter; then he would assign to them the value of a whole syllable, or even of two syllables, without troubling himself with the strangeness of this mixture of characters so different in their nature. The fragment of the alphabet published by Dr Young, contains both what is true and what is false; but the false so much superabounds in it, that it would be *impossible* to apply the significancy of the letters of which it is composed, to the reading of any thing else than the two proper names from which it was derived. The word impossible, however, is so rarely applicable to the scientific career of Young, that I hasten to prove my assertion. I remark, then, that, subsequent to the composition of his alphabet, Dr Young himself discovered, within one of the frames upon an Egyptian monument, the name *Arsinoe* where his celebrated competitor has since deciphered, to the entire satisfaction of every one, the word *Autocrator;* and, in the same way, Dr Young read *Evergete* in a group which contained the name Cesar.

The labours of Champollion, in reference to the discovery of the phonetic value of hieroglyphics, is simple, homogeneous, and apparently does not afford ground for any uncertainty. Each symbol is the equivalent of a simple vowel or a simple consonant. Its value is not at all arbitrary; every phonetic hieroglyphic is the image of some physical object, the name of which, in the Egyptian language, commences with a vowel or consonant, which it was the object to represent.

The alphabet of Champollion, once formed from the pillar of Rosetta and two or three other monuments, serves for the deciphering of inscriptions which are wholly different, as, for example, for the name *Cleopatra*, upon the obelisk of Philæ, long ago removed to England, and where Dr Young, using his own alphabet, could make out nothing. Upon the temples of *Karnac*, Champollion twice read the name of Alexander; upon the zodiac of Denderah, he read a Roman imperial title; upon the great edifice above which the zodiac was placed, he read the names and surnames of the Emperors Augustus, Tiberius, Claudius, Nero, Domitian, &c. And thus, we may remark in passing, is decided, on the one hand, the stirring and endless discussions concerning the antiquity of these monuments; and, on the other, it is definitely determined that, under the dominion of the Romans, hieroglyphics were still in general use upon the banks of the Nile.

The alphabet which has already afforded so many unlooked for results, whether applied to the grand obelisks of Karnac, or to other monuments which are also known to be contemporaneous with the Pharaohs, will present us with the names of many of the kings of this ancient race, with the names of their divinities,—we will say more, with substantives, adjectives, and verbs of the Coptic language. Dr Young, then, was deceived, when he supposed the phonetic hieroglyphics to be a modern invention, and when he advanced that they had only been employed in the transcription of proper names, and those proper names only which were foreign to Egypt.

M. de Guignes and especially M. Etienne Quatremere have, on the contrary, established a fact of the greatest importance, which the reading of the inscriptions of the Pharaohs has corroborated by irresistible proofs, since they have pointed out that the present Coptic language is the same as that of the ancient subjects of Sesostris!

And now I have adduced the facts, and may therefore limit myself to establish, by some short observations, the consequences which appear to me to result from them. Discussions about priority, even under the influence of national prejudices, would never become embittered, if it were possible to determine them by fixed rules; but in one case the first idea is every thing, whilst in another the details offer the principal difficulties, and sometimes the merit seems to consist less in the formation of a theory, than in its demonstration. These reflections shew how much the selection of the point whence the subject is viewed will lead to a capricious result, and how much influence it will likewise have upon the ultimate conclusion. To escape from this embarrassment, I have sought for an example in which every one would agree;—in which the parts performed by the two claimants to the discovery, might be as near as possible assimilated to those performed by Young and Champollion. This example, I think, I have found in the question of *interferences*,* even when, in the hieroglyphic discussion, we entirely lay aside the citations which were taken from the memoir of M. de Guignes.

Hooke had in truth affirmed before Dr Young, that luminous rays interfere, as Dr Young had supposed before Champollion that the Egyptian hieroglyphics are sometimes phonetic. Hooke did not directly prove his hypothesis; and the proof of the phonetic values assigned by Young to different hieroglyphics, could only repose upon readings which had not, and could not, be made. From his ignorance of the composition of white light, Hooke had not an exact idea of the nature of interferences, as Young, on his part, deceived himself concerning a pretended syllabic and disyllabic value of hieroglyphics. Young, by unanimous consent, is considered as the author of the theory of interferences; and hence, as it appears to me, by inevitable consequence, Champollion ought to be regarded as the author of the discovery of hieroglyphics.

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* Under the name of *interference* is understood a very remarkable action of the rays of light, in virtue of which two of these rays, at their point of crossing, may sometimes be united, and sometimes may completely destroy each other. By this law of interference, therefore, the optician arrives at a result which is almost incredible; he may produce *a total obscurity by adding light to light!*

I regret I did not sooner think of this method of agreement. If Dr Young whilst living had been offered the alternative of being considered the author of the doctrine of interferences, making over the hieroglyphics to Champollion, or of keeping the hieroglyphics, and giving over to Hooke the ingenious optical theory, I have no doubt he would have been all anxiety to recognize the claims of our illustrious compatriot. Even then there would have remained to him, what none would have disputed, the right of bearing in the history of the memorable discovery of hieroglyphics, that part which Kepler, Borelli, Hooke, and Wren, bear in the history of universal gravitation.

Remarks on the Garnkirk and Glasgow Railway. By Mr GEORGE MARTIN, Edinburgh. *

RAILROADS, notwithstanding the visionary objections of certain theorists, and the prejudices of a few individuals denominating themselves practical men, are gradually securing for themselves the approbation and support of the enlightened portion of the community.

The Stockton and Darlington, the Liverpool and Manchester Railways, two of the greatest undertakings of this description yet opened for the benefit of the public, have deservedly attracted the attention of all who feel interested in the advancement of science and commerce.

Much has been written on their construction, and not a little

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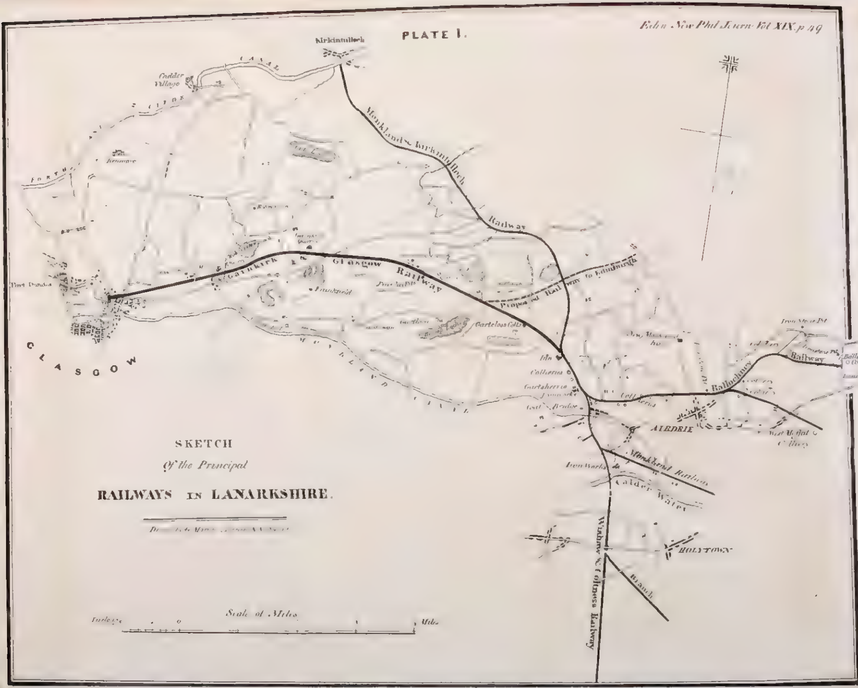
* Read before the Society of Arts for Scotland, 22d April 1835.

Edin. New Phil. Journ. Vol. XIX. p. 49

PLATE I.



SKETCH

Of the Principal

RAILWAYS IN LANARKSHIRE.

discussion has been excited as to their ultimate success, which has justly been looked upon as the test of the expediency of forming others in various parts of the kingdom.

Their success, however, having more than equalled the most sanguine hopes of their proprietors, has satisfactorily demonstrated, that such undertakings must not only ultimately prove a safe and profitable investment for capital, but likewise be of essential benefit to the empire.

In the course of my enquiries on the subject of Railways in England, I was naturally led to consider the extent of such undertakings in this part of the island. These I find have, comparatively speaking, advanced but little; the greatest works of this description being situated in Lanarkshire, in the vicinity of Glasgow, and in one of the richest mineral districts of Great Britain.

The public Railways constructed in this quarter are the Garnkirk and Glasgow, the Monkland and Kirkintilloch, the Ballochney and the Wishaw and Coltness, the last of which is not yet finished. (Plate I.)

Of all these undertakings, the Garnkirk and Glasgow railway, both from the magnitude of the works connected with its formation, and the superior manner in which it is constructed, more particularly attracted my attention.

The apparent object of this work was to form a communication with the Glasgow market, it being clear to every one, that if coals could be sent for a distance of 17 miles (one-half of which was by a railway conveyance, and the other by water) with any chance of profit, both the proprietors and the public would be gainers, if a railway conveyance could be got for the whole distance. Besides this, the greater dispatch obtained by shortening the distance one-half, and its being the means of bringing into the market many new fields of coal and ironstone, which, from the difficulty and expense of carriage, it was thought a needless waste of money to attempt to work; were additional inducements to the promoters of the undertaking.

From the inequalities of the face of the country through which this line of railway has been carried, and from the well-known maxims of engineering, that the least deviation from a level line adds considerably to the difficulty of draught; and that great sacrifices ought to be made, rather than resign that line which is to admit of the greatest facility of conveyance; the construction of this line of railway became a serious undertaking.

The work was begun in 1827, was finished and opened to the public in September 1831.

The length is $8\frac{1}{4}$ miles, and the fall is 95 feet. For the first 5 miles or thereby, from the east end of the line, it is on a level; but then falls at the rate of one foot perpendicular to 144 horizontal.

The average radius of the different curves on the line is about 6000 feet. Plate I.

In the execution of the work, great labour was required, owing to the many heavy embankments and cuttings.

The largest embankment is through the lands of Germeston and Rosebank. Its length is about 4000 feet, and its height for a considerable distance between 40 and 50 feet. Its breadth at the base is 200, at the top 30 feet. It contains about 500,000 cubic yards of stuff.

Another embankment through the lands of Gartcloss is about 4000 feet in length, and in some places 30 feet in height. It contains about 140,000 cubic yards.

One through the lands of Gartclosh, containing about 60,000 cubic yards, is about 2500 feet in length, and in some places 15 feet in height.

These two last embankments were very difficult to form, as, from the ground being soft and mossy, the stuff did not acquire firmness until it had sunk far below, and displaced a considerable portion of the original surface.

The cuttings are also very considerable, varying from a few hundred feet to 4000 in length. In some of the cuttings, the quantity of earth removed was upwards of 300,000 cubic yards. The natural materials of the ground in which the greater portion of the heavy cuttings were made, being very hard and tough, and the distance they had to be conveyed being considerable, it required great perseverance to get through with this part of the undertaking.

The cuttings through Robroyston Moss is about 2700 feet in length, and 30 feet in depth; so soft and spongy was the

PLATE II.

Ed. New Phil. Journ. Vol. XIX. p. 31

Fig I

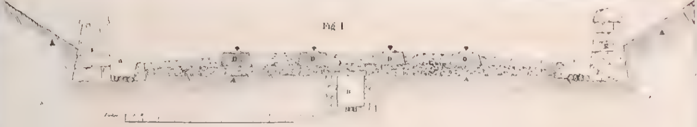
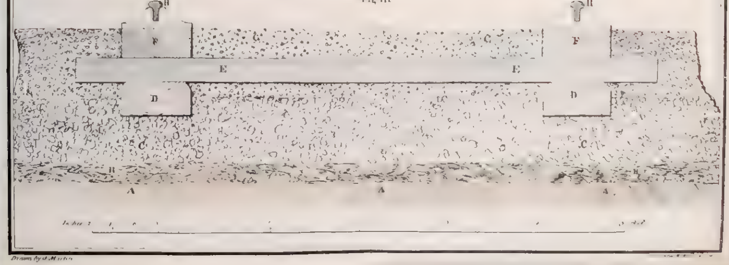


Fig II

Fig III



Drawn by J. Martin

ground, that when a few feet in depth had been cut out, the ground on each side frequently slipped in, while the operations were going on, and thus this cut may be said to have been three or four times excavated. The ground on each side of the line is not now more than 5 or 6 feet above the surface of the rails. The whole body of the moss, to the extent of about 400 feet on each side the railway, has sunk more than 20 feet, chiefly from the water having been drawn off by the operations in the formation of the railway at this point. Plate II. fig. 2. represents a section of the railway, and of the original surface of the ground at this point. The dotted line GG, the original surface of the ground; AAA its present surface. Figs. 2, 3, point out the manner adopted in forming the railway, and in laying the rails through this and some other mosses on the line, which is different, I believe, from any other yet tried in such situations. AAA surface of ground when brought to the requisite level; BB hurdles of brushwood and whins; CCCC ballasting and boxing laid over the whins; on the ballasting, the rough longitudinal beams of timber DD are laid, varying in length from 20 to 40 feet; breadth 9 inches; in thickness 4. EE cross beams or sleepers, laid on the longitudinal beams at the distance of, from a few inches to 3 feet apart, as required. FF longitudinal planks 4½ inches thick, 9 inches broad, and 40 feet long, laid on cross sleepers, and on which the chairs HH are fixed.

Fig. 1. is a section of the railway where it passes through other cuttings AAAA, surface of ground at the requisite level; CC ballasting and boxing; DDDD stone-blocks or pedestals on which the chairs rest, and are 10 inches in depth, 2 feet by 1 foot 6 inches at the base, and tapered to 1 foot 6 inches, by 1 foot at the top, weighing about 3 cwt. In these stones holes are bored ⅞ths of an inch diameter, and 4 inches long; the chairs are secured to these stones by means of oak pins, driven into these holes through holes in the chairs. The rails are fixed to these chairs by means of iron keys, or wedges; at the extremities of the rails they are united by half-lap joints EE stone-walls on each side of the railway; B drain under the railway, varying in size as the nature of the soil requires.

The dotted lines on Fig. 1. marked *a a a a*, shews the section of the sides of the railway through embankments.

The roadway for the railway is everywhere at least 30 feet wide. The centre part is occupied by the rails laid in two lines; at some places increased to three. The rails, placed 4 feet 6 inches apart, are from 5 to 6 yards long, 2½ inches broad on the top, 4 inches deep in the centre, 2½ inches at the ends.

The greater portion of the rails on this line weigh 28 lbs.; but, in some places, especially through the mosses, the weight is 36 lbs. per yard; they are all of that description called fish-bellied, except those laid in Robroyston Moss.

I believe, when locomotive engines are generally to be used on a railway, rails weighing 40 lbs. per yard are not considered too heavy.

The depot at the western termination is about 12 feet below the level of the railway. A framing of cast-iron, supported by cast-iron pillars and beams, is raised to the requisite level, on which rails are placed. There are hatchways in this framing for allowing the coal to be emptied through into the sheds below, keeping the railway waggons quite distinct from carts and other conveyances on the common roads, and thereby preventing the confusion and accidents which otherwise might have occurred, besides being more convenient.

The ground occupied by the operations extends to 80 imperial acres or thereby.

The railway cost, up to the 31st of December last, L. 87,371, being at the rate of L. 10,590 per mile. Depots, L. 7028. Locomotive engines, waggons, coaches, &c., about L. 10,000. The keeping of the line in proper order was let last year for L. 720, but it is expected that the maintenance this year will be less by L. 100. Since the opening of the railway, it is gratifying to see that the trade on it is steadily increasing.

In the printed Report for 1834 by the Committee of Management, it is stated as under:—

Years.	No. of Tons.	Increase.	No. of Passengers.	Increase.
1832,	114,144	—	62,605	—
1833,	112,471	—	96,003	33,398
1834,	132,657	20,186	117,743	21,740

The conveyance of goods on this railway is carried on by means of locomotive engines, of which there are four (the

George Stephenson, the Glasgow, the Garnkirk, and the Gargill) presently on the road, besides a new one put on a few weeks ago for a trial. The oldest one is the George Stephenson, made by Messrs Stephenson, Newcastle. Its weight is 6½ tons, exclusive of the water in the boiler. It has two cylinders 11 inches diameter and 16 inches stroke. Boiler 3 feet 3 inches diameter, 7 feet long, exclusive of the fire-box, which is surrounded by water. The power of this engine is calculated to be about 30 horse. The ordinary loads drawn by this engine is 80 tons, but it has frequently hauled upwards of 110 tons at the rate of 12 miles an hour. This engine, with a train of empty waggons and a carriage with nine passengers, weighing in all about twenty tons, travelled at the rate of one mile in one minute and a quarter, being at the rate of 48 miles an hour.

The Glasgow engine, nearly similar in construction to the George Stephenson, weighing 8½ tons, its power is calculated at 30 horse, and the usual load drawn by the engine is 90 tons; it has, however, drawn 140 at the rate of 7¼ miles an hour. The Garnkirk engine, made by Murdoch and Aiken, Glasgow, weighs 7 tons 16 cwt. Its power is about 30 horse, and it is similar in construction to the last two. The Gargill engine, likewise made by M. and A., weighs 11 tons; its cylinders are 13 inches diameter, and its stroke 18 inches. Its power is calculated at about 40 horse, and its usual loads is from 30 to 40 waggons, weighing 4 tons each; on one occasion it hauled a train of 70 waggons, weighing in all 287½ tons, a distance of 8 miles in one hour and five minutes.

During last year, this engine performed about three trips per day of 16½ miles, one half loaded and the other half empty waggons. Reckoning 310 working days in the year, we have about 111,000 tons conveyed by this engine along the whole line, or about 1,370,000 tons conveyed one mile.

The cylinders of the new engine are 13 inches diameter, and 18 inches stroke, and its power is 40 horses.

All the engines have two safety valves, one only under the control of the engineer. The usual working pressure is from 45 to 50 lbs. per square inch. The cylinders of the Gargill engine are placed at an angle of 45 degrees; the cylinders of the other three are horizontal, and those of the new engine vertical. All the cylinders have metallic packing; the boilers are all of that description called tubular. In the Gargill there are 82, 6 feet 6 inches long, 1¾ inches diameter interior. In the Garnkirk 107; in the George Stephenson 104; in the Glasgow 102, all 1⅝ inches diameter; in the new engine 92, 1¾ inches diameter, the length of all the tubes, except those in the Gargill, is 7 feet, exclusive of the water casing round the fire-box. The average quantity of water in the boiler in an ordinary working state is about 300 gallons, weighing 26 cwt. 3 quarters and 10 lbs.

When the water is quite cold, the fire new, and the engine stationary, it requires one hour to raise the steam. When the engine has been in use, and the water in the boiler been partly used the previous day, and the engine stationary, steam is raised in three quarters of an hour. When the engine is run out to create a draught, steam is raised in half an hour.

There is no restriction on this railway, as on the Liverpool and Manchester, as to the description of fuel to be used. The best splint coal of the Monkland district is employed, the price of which is 5s. 6d. per ton, being less by two-thirds than the expense of coke. This produces a great saving as compared with the English railways. It has also been ascertained that one ton of this splint coal produces as much steam as the same weight of coke. Moreover, there is a great saving of the tubes in the boilers of engines where this coal is used, arising from the formation of a crust in their interior, which preserves them, and has no perceptible injurious effect in preventing the full action of the heat on the water in the boiler. There have been as yet no decided experiments made to prove this statement, but it may be inferred to be correct from the fact, that, on this railway, of the whole of the boilers of the engines since its opening, only eight tubes have given way, while on the Liverpool the tear and wear have been enormous, and the expense correspondingly great.

As far as can be judged, this undertaking holds out the prospect of fully realizing the expectations of its projectors. This, and similar undertakings in the neighbourhood of Glasgow, have had the most beneficial effects on the manufactures and shipping of that city; for, supposing that they have reduced the price of coal only one shilling per ton (I believe it is even more), it will give an annual saving of L. 50,000.

The progress which such works are making in England holds out a good example for the encouragement of the same in this part of the country.

It is to be hoped, that when the public are made fully aware of the great advantages to be derived from such undertakings, we shall soon see works of this description intersecting all the populous districts of Scotland. I have no doubt that this will ultimately be the case, and that thereby the many rich mineral fields of the country will be rendered available; while its commerce will be extended, and the comforts of the labouring population proportionally increased.

EDINBURGH, 15*th April* 1835.

I was at the meeting of this Society when Mr David Stevenson's "Observations on the Liverpool and Manchester Railway" were read. He stated that it required one hour and twenty minutes to raise the steam. Mr Grainger, one of the members, stated that the time was generally from twenty minutes to half an hour; the difference of opinion being so great, I was anxious to ascertain the exact time required. I therefore wrote to Mr A. Fyfe, superintendant of the locomotive engines on the Liverpool and Manchester Railway, and beg leave to annex the following excerpt from his letter.

EXCERPT.

'MANCHESTER, 7*th March* 1835.

'DEAR SIR,—Your's of the 4th I received, on the subject of getting up the steam in the boilers of locomotive engines. At the engine-house here, the night watchman puts into the fire-box the full of his arms of timber, in general old oak sleepers, which are (as you must be aware) in a wet state. He then takes from a large furnace a few shovelsful of coke, then fills the fire-box with dry coke; this is done one hour and a half before the engine is to be used, with from four to six inches more depth of water in the boiler than the engine runs with. With dry deal and coke one half hour is sufficient. The engines sent from

England to South Carolina, by using the dry deal of that country, the steam is got up in eleven or twelve minutes.

I may mention, that much depends upon the situation the engine stands in, so as to catch a current of air. Many other reasons may be given; for instance, a new boiler will get up the steam in a shorter period than an old one. Moreover, much depends likewise on the nature of the water; the water we use has lime in it, which crusts the outsides of the tubes rather thicker than an egg-shell; it breaks off and rests in the bottom of the fire-box; it has to be cleaned out by a force-pump every fourteen days; it will in that time be six inches thick.

At the Leaster Railway the water was so bad, that the tubes got a strong store tube on the outside; and had they not got better water, they must have given up locomotive engines.

Account of some of the Rarer Plants observed during an Excursion in the United States and the Canadas in 1834. Communicated by Mr JAMES MACNAB.*

FEW countries can afford a greater treat to the lover of botany than North America. Its vast extent of diversified surface, and the extremes of climate, cause a peculiarly interesting series in its humbler vegetable productions, which are spontaneously scattered throughout its woods and swamps, its hills and river-sides, its pine barrens and rich prairies, in a manner harmonizing well with the surrounding scenery. In spring great portions of the country are covered with the blossoms of interesting dwarf-flowering plants; and as the season advances, more luxuriant herbs take their place; these are again succeeded in the autumnal months by numerous species of Compositæ, forming at that period the greatest bulk of the herbaceous vegetation of these regions. Through vast tracts of country, where the surface has not yet been disturbed, the distribution of the native plants is remarkable; many of them seem very irregularly scattered and occupy mere spots at one place, while others are extended over a great extent of surface in dense masses, one species being frequently predominant for many miles together. Every day's travelling presents different and leading botanical objects of interest. The extent of country traversed, however, and the number of species observed, render it impossible here to give any thing like a precise statement of the distribution of all the species that came under our notice, and the appearances which they present in their native soils and situations, compared with what we are accustomed to see of them in cultivation. We shall therefore enumerate a few only of those species which were regarded as scarce, or whose localities have hitherto been doubted, or not known to exist, in those tracts of country where we had the good fortune to meet with them. We have the pleasure also to add something new to the American Flora.

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* Read before the Wernerian Natural History Society, 25th April 1835.

With the exception of one of the New Jersey swamps, the portion of the country from which we reaped the greatest harvest of botanical rarities was the neighbourhood of Goderich in Upper Canada. The month of August having nearly closed before we reached this last place, and most of the summer-flowering plants having passed into seed, a year or two must in consequence elapse before we can satisfactorily identify some species, either by seeds or roots collected on the spot and cultivated in Scotland. Goderich, in the district of Colburn, lat. 43° 30′, long. 81° 42′ west, is situate on the east bank of Lake Huron, seventy-two miles above its outlet into the River St Clair. Though the town has not been established more than nine years, it was gratifying to witness the degree of perfection which it, as well as the surrounding country, has already attained, owing to the industrious and persevering habits of the settlers, who are mostly Scotch. If the same habits of activity continue with the increasing population, it bids fair to become one of the most interesting of the Canadian settlements. The neighbouring grounds are rich, beautifully undulated, and well watered by numerous streams running into the Maitland River. The entrance of this river into Lake Huron forms the harbour at Goderich, but the river is not navigable above the town. The scenery along its banks is diversified and picturesque, having at one place steep clay banks well wooded, at another, moist limestone rocks, with extended gravelly and meadow grounds adjacent. One great advantage which this neighbourhood possesses, over the other tracts of Canada which we traversed, is, the very desirable nature of its soil; being easily cleared and easily cultivated. The fine natural undulations before alluded to, are likewise very advantageous; for commanding situations can thus be procured, on which to place the dwelling-houses, so as to have a view of the country around, whereas, in the generality of cases the surface is nearly level, and there is no possibility of seeing beyond the space from which the trees are cut down.*

RANUNCULACEÆ.—*Cimicifuga racemosa*, Nutt. and Torry. Abundant on both sides of the banks of the Genisii river, leading from Lake Ontario to Rochester, United States.

Cimicifuga podocarpa, Dec. On the sides of moist wooded valleys near the summit of the Allegheny range, where it was seen in quantities intermixed with *Cimicifuga serpentaria*, from which it is readily distinguished by its stipitate germens, which are in clusters varying from four to six.

VIOLARIÆ.—*Solea concolor,* Dec. Moist meadows, Goderich Road, Upper Canada.

CISTINEÆ.—*Hudsonia ericoides, var. tomentosa,* Nutt. Dry, sandy, barrens; New Jersey. We are much inclined to think that this is only a variety of *H. ericoides ;* they were both seen in abundance together, and many sub-varieties were observed which seem to unite the two. This curious shrub covers large tracts of the dry white sandy plains of New Jersey, in large round tufts, where it resembles very much the common ling (*Calluna vulgaris*) of Scotland.

DROSERACEÆ.—*Parnassia caroliniana,* Mx. Banks of the river Thames, New London, Upper Canada.

LINEÆ.—*Linum virginicum,* Linn. On the moist banks of marshes, Peninsula of Toronto, Upper Canada.

HYPERICINEÆ.—*Hypericum frondosum,* Mx. and Schweinitz. Moist shady places along the banks, on the Canada side, of the St Clair river.

Hypericum Kalmianum, var. elongatum. Moist places; Quaker Bridge; New Jersey. The *H. Kalmianum* we saw in great abundance in the neighbourhood of Niagara Falls, growing on dry spots near the Table Rock; whereas the New Jersey plant was found in low moist places, and differs from the other by its branches being more elongated, and the leaves rounder at their extremities. The flowers are scarcely so large, but as rich in colour as in the Niagara plant.

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* I may mention that the trip was undertaken chiefly for the gratification of my valued friend Mr Brown, formerly of the Perth Nurseries, whose ardent love of science led him to America, that he might have the opportunity of seeing, in a state of nature, those forest trees and shrubs, to the rearing of which, in early life, his attention had been devoted; and a greater treat need not be sought for by any one whose delight is in the cultivation of plants or shrubs, than to see the perfection to which they attain in their native soils: Thus, too, will he be the better able to judge of the degree of perfection to which they are capable of being brought by cultivation. The tour, therefore, was strictly private, and not (as some of the public journals seem to have imagined) under the auspices of any public institution.

Leguminosæ.—*Lespideza hirta*, Torrey. Dry moorland ground near Hamilton, Upper Canada.
Lespideza frutescens, Elliot. Dry moorland ground, Brentford, Upper Canada.

Rosaceæ.—*Potentilla fruticosa*, Linn. *Potentilla floribunda*, Pursh. Banks of the Maitland river, Goderich, Upper Canada. This plant agrees with Pursh's description of *P. floribunda*, distinguished principally from the *P. fruticosa* by its being a low-growing shrub, varying from 5 to 18 inches in height, and having the leaves and flowers more numerous. Few plants in beauty could excel this variety of *Potentilla*, when seen as it was by us in full flower, (August 1834), covering vast tracts of level ground about three miles above the mouth of the Maitland river. It is a low-growing shrub, not exceeding 9 inches in height. The flowers are small, but of a beautiful bright yellow. The leaves in general are very narrow and densely coated with white silky hairs. If this plant shall retain the same habit in cultivation which it exhibits in its native soil, it will prove an interesting addition to our gardens.

Halorageæ.—*Myriophyllum heterophyllum*, Mx. In stagnant pools by the side of the Maitland river, near Goderich, Upper Canada.
Hippuris vulgaris, Linn. In pools by the side of the Maitland river, near Goderich, Upper Canada.

Saxifrageæ.—*Heuchera pubescens*, Pursh. In the exposed crevices of rocks; on the summit of the Allegheny mountains.

Umbelliferæ.—*Archemora rigida*, Dec. Marshy places on the banks of Lake Ontario, near Kingston, Upper Canada.

Rubiaceæ.—*Symphoria occidentalis;* spicis densis terminalibus axillaribusque nutantibus, corolla intus laciniisque dense barbatis, stylo staminibusque subexertis. (Richardson in Franklin's First Journey.) Meadow lands near Goderich, Upper Canada. After a careful examination, we find that this plant agrees perfectly with the foregoing description, and also with the specimens in the herbarium of Dr Hooker from the Saskatchawan and Red rivers. This beautiful shrub covered a great proportion of an extended rich natural meadow lying on the north side of the Maitland river, near Goderich, where it was growing in bushes not exceeding 2½ feet in height. They presented a curious appearance, bearing at the time (August 1834) both flowers and fruit; their large, handsome, and somewhat reticulate leaves proving also very ornamental.

Valerianeæ.—*Patrinia longifolia*, *nov. sp.* caule glaberrimo, foliis ciliatis lanceolatis integris vel remotè pinnatifidis subparallelis nervosis radicalibus infernè longissime attenuatis, caulinis perpaucis angustis, panicula (immatura) congesta. On the banks of the Maitland river, near Goderich, Upper Canada. August 1834.

Of the natural order *Valerianeæ*, numerous as are the species in the temperate regions of South America, very few are enumerated as indigenous to any part of North America. Two only, *Valeriana pauciflora* and *Fedia radiata*, are mentioned in Nuttall's Flora. Sir Joseph Banks detected a third species in Newfoundland, *V. sylvatica*; Chamisso and Dr Richardson a fourth in the Arctic regions, *V. capitata;* and the late Mr Douglas two others, both on the west or Pacific side of the Rocky Mountains, *Plectritis congesta*, and *Patrinia ceratophylla.* It is to the latter genus that we refer with little hesitation the present plant, for, although we found its flowers only in bud, the herbage and roots possess entirely the characteristics of *Patrinia*. It is quite distinct from any hitherto described, all of which, with the exception of the one just mentioned, have yellow flowers, and are natives of temperate or Northern Asia. The root is fusiform and woody, thicker than a swan's quill, and, when bruised, yields the same smell as that of *Valeriana officinalis ;* its neck is sheathed with large glossy dark brown scales; the broken bases of the leaves of former years; and from within these arise several radical leaves, from 8 to 10 in number, the longest of them nearly a foot in length, not much unlike those of a *Scorzonera*, or a *Troximon*, lanceolate, rather obtuse, entire or deeply pinnatifid, with 1–4 very remote, linear, spreading segments ; the base is much attenuated, but scarcely into a petiole. Both surfaces of the leaves are glabrous, but the margins are finely ciliated with pubescence; there is one principal nerve or midrib, and a few lateral and almost parallel ones, and besides, the whole surface of the leaf, when held up between the eye and the light, is seen to be marked with minute reticulated nerves. The stem, at the stage of growth when our specimens were gathered, was not more than 4 inches high, bearing two pinnatifid ciliated leaves, similar to those of the root, and a dense rounded head or panicle of flower-buds. These, when recent, were of a whitish colour, and the flowers are probably similar to those of the only other American species, *P. ceratophylla*, Hook. Fl. Bor. Am., vol. i. p. 290. This we regard as a very interesting addition to the Canadian Flora.

Compositæ.—*Prenanthes racemosa*, Mx. Banks of the Maitland river, Goderich, Upper Canada. This is very abundant amongst the moist grass on the banks of the river, where it attains the height of 14 inches. This species is entirely confined to the northern parts of Canada, and is, without exception, the most interesting species of this extended genus which we met with.

Prenanthes alba, Linn. Shady woods, Goderich, Upper Canada.

Liatris scariosa, Willd. Dry sandy grounds, St Clair Bay, Upper Canada. Few plants exceed in beauty this species of *Liatris*, particularly when seen in such quantities as were witnessed by us during September 1834, covering several acres of dry loose sandy ground, at the place where the river St Clair issues from Lake Huron. The specimens varied from 2 to 4 feet in height, having dense crowded flower-spikes from 4 to 16 inches long, and of a bright purple colour. All the species of *Liatris* which we saw were growing in situations more or less stony or sandy, with the exception of *L. macrostachya*, which was found in deep rich soils on the borders of woods, both in New Jersey and Pennsylvania.

Liatris spicata, Willd. Stony places on the banks of the Maitland River, Goderich, Upper Canada.

Liatris pilosa, Willd. In a white sandy pine forest between Long-a-coming and Atsion ; also in a similar forest between Quaker Bridge and Tuckerton, both in New Jersey.

Liatris borealis, Nutt. Herb. M.SS. ; caule simplici pubescente, foliis radicalibus ovato-lanceolatis acuminatis longe petiolatis, caulinis superne sessilibus utrinque attenuatis marginibusque scabris, involucri squamis inferioribus brevibus obovatis marginibus coloratis, interioribus linearibus longitudine flosculorum, capitulis sublonge pedunculatis alternis remotis distichis, superioribus erectis, inferioribus patentibus. *Hab.* Dry woods and exposed rocky places throughout the Allegheny Mountains. This species is readily distinguished from the *L. scariosa*, (which by some is supposed to be only a variety), by its being very much smoother, having fewer leaves and flowers, and reflected bracteas ; also by its short styles, which scarcely exceed the corolla ; whereas, in the *L. scariosa*, the styles are long, and the leaves, flowers, and bracteas, all upright.—Very abundant throughout the elevated regions of the Allegheny Mountains, where it attains the height of 14 inches, and having from two to eight heads of large light purple flowers. While looking through the herbarium of Nuttall when at Philadelphia, I found that this species was marked *L. borealis ;* but no description of it has as yet been published.

Liatris stricta ; caule rigido obscure angulato, foliis inferioribus elongatis lineari-lanceolatis superne sensim minoribus bracteiformibus, spica brevi pauciflora, involucro longo cylindrico, squamis ovatis acuminatis mucronatis, pedunculis brevibus erectis.—*Hab.* On dry sandy pine barrens, Brentford, U. C. The tuber is small and globular; the stem varies from 8 to 12 inches in height ; the lower leaves are about 5 inches long and ¼ of an inch broad, and shining. Florets few, and of a bright purple. The leaves and flowers are all upright, and, with the stem, hard and rigid. This interesting species adorned the road-sides for several miles through the pine barrens to the eastward of Brentford, where we collected many flowering specimens during August 1834. On showing this species to Dr Torrey of New York, he informed me that he lately received specimens of it from Canada, and purposes calling it *Liatris cylindracea.*

Chrysopsis alba, Nutt. Dry hilly ground round the head of Burlington Bay, Hamilton, Upper Canada.—This plant was gathered in abundance at the above station, amongst short grass, and in old burnt woods where the surface was hard and dry. The plants were growing singly, two being seldom seen within a foot of each other; it is readily distinguished at first sight from all other species of the genus by its lanceolate rigid leaves, and simple wiry stem, which never exceeds 1 foot in height.—This new addition to the Canada Flora has hitherto been only noticed as inhabiting the banks of the Missouri river near Fort Mandan.

Cacalia tuberosa, Nutt. Banks of the Maitland river, Goderich, U. C. In shady situations, bordering on the Maitland river, this plant was seen in great abundance. Those first observed by us, not being in flower, were passed for a species of *Plantago*, whose leaves it very much resembles. Proceeding farther up the river we met with the plant in greater quantities, and in full flower. The only stations known for this plant previous to our finding it in Canada were on the banks of the Missisippi, about Natchez, and near St Louis. Nutt. Gen. Plant.

Rudbeckia hirta, Linn. Dry sandy banks, Peninsula of Toronto. This plant is mentioned by Pursh as a native of the mountains of Virginia and Carolina. It presented quite an alpine appearance, varying from 2 to 12 inches in height, and densely coated with rough hairs.

CAMPANULACEÆ.—*Campanula linifolia*, Hænk.—Caule sub-3-floro, foliis lanceolato-linearibus subintegerrimis subsecundis, summis angustissimis, floribus cernuis. *Spr. Syst. Veg.—Hab.* Dry barren places, St Clair Bay, Upper Canada. Besides this station, this plant was also seen, though sparingly, on the Peninsula at Toronto. Its dark blue drooping flowers render it an interesting object in such wildernesses.

LOBELIACEÆ.—*Lobelia cardinalis, alba.* Swampy places, Goderich Road, U. C. The common scarlet kind covers large tracts of moist places in many parts of the country; but in no district did we witness it in such perfection as on the marshy grounds leading from new London to Goderich. Out of such quantities it was really surprising to see so few varieties. A small number were observed of a delicate white, and some intermediate between that and scarlet, but the white variety was by no means plentiful.

ERICEÆ.—*Arbutus Uva-Ursi.* This plant was seen in great abundance on dry barren situations in the neighbourhood of the great lakes, as also on the pine barrens of New Jersey. But, like the *Linnæa borealis*, *Azalea procumbens*, &c. &c. it differs from the British type in having much larger leaves and fruit, and being of more upright growth.

PYROLACEÆ.—*Pterospora Andromedea*, Nutt. In beech woods, west of Kingston, Upper Canada.

GENTIANÆ.—*Gentiana alba*, Muhl. Cat.; caule simplici tereti glabro, foliis ovatis acuminatis amplexicaulibus trinerviis, floribus terminalibus sessilibus, laciniis calycinis late-ovatis acuminatis reflexis, corolla ventricosa 5-fida, laciniis internis parvis denticulatis.—*Hab.* Dry stony places, Allegheny Mountains. Two beautiful specimens in the Muhlenberg Herbarium, now in the possession of the Lyceum of the Natural History Society at Philadelphia, are identical with those found by us on the Allegheny Mountains on the 15th of September 1834; but whence they came is unknown. At this time the plant was in full flower; it was growing in robust tufts, with stems 1 foot high, having opposite amplexicaul leaves, and crowned with a large head of white bell-shaped flowers.—While looking through the species of *Gentiana*, in the recent collections of Mr Drummond, I observed some beautiful specimens of this plant from the neighbourhood of St Louis, differing only in their taller and more slender growth, which may be attributed to the situation in which they were procured.

Gentiana angustifolia, nana. Dry pasture grounds, Tuckerton, New Jersey. The *G. angustifolia* we first found in the *moist* woods in the neighbourhood of Quaker Bridge, New Jersey, where it attains the height of 14 inches. Nothing more was seen of this plant till we reached Tuckerton, where it proved abundant, and interesting from the circumstance of its clothing a large tract of natural *dry* pasture ground bordering upon the salt marshes. The greater proportion of the specimens varied from 2 to 4 inches in height, and none exceeded 5 inches. A careful examination of this plant, in the hopes of finding characters to distinguish it from the *G. angustifolia* of the moist woods, proved fruitless; the only marked difference being in the stamens, which were much longer. Besides its small size, many of the plants were much branched, and had from 1 to 5 flowers. The colour of these was exceedingly various, being sometimes perfectly white, and having all shades between white and blue; whereas, out of many hundred flowering specimens of wood plants, not the slightest deviation in tint was perceptible, all being of a beautiful rich blue dotted with yellow. In the dwarf white flowering varieties the dots were green.

Gentiana barbata; caule erecto tereti-angulato ramoso, foliis lineari-acuminatis linearibusve, radicalibus spathulatis, calycis inæqualis laciniis lanceolatis longe acuminatis margine membranaceis, corollæ 4-fidæ laciniis antice serratis medio inciso-ciliatis.

α. sibirica; corollæ laciniis apice subintegerrimis, foliis lineari-lanceolatis sensim acuminatis. *Gentiana barbata*, Irvel. Gent. p. 114. Ledeb. Fl. Alt. v. i. p. 282. *Gentiana ciliata*, Pall. Fl. Ross, v. ii. p. 101. t. 92. Sims in Bot. Mag. t. 639.

β. Browniana, Hook. MSS.; corollæ laciniis apice evidenter serratis, foliis lineari-acuminatis.

γ. Richardsoniana; corollæ laciniis apice evidenter serratis, foliis linearibus obtusis. *Gentiana crinita*, Rich. in Frankl. 1st Journ. App. ed. 1. p. 734. *Gentiana intermedia*, Rich. in Frankl. 1st Journ. App. ed. 2. p. 9. (not Sims Bot. Mag. t. 2303.)

It is the var. β alone of this plant which we found at Goderich, growing on the banks of the Maitland River, which it adorned with its copious and large pale blue flowers. It is so completely the connecting link between the Asiatic *G. barbata* and the *G. intermedia* of Dr Richardson, that, after a careful comparison with numerous specimens, in all states, in Dr Hooker's herbarium, we cannot but consider them as slight deviations of one and the same species. The northern Asiatic plant seems to have constantly more obsolete serratures at the tips of the corolla; but even there the leaves are liable to vary in form; a specimen from Kamschatka having them as truly *linear* and *obtuse* as any of Dr Richardson's specimens. In our state of the plant, which is exactly the same as the late Dr Todd's specimens in Dr Hooker's herbarium, the serratures are strong at the apex of the segments of the corolla, but the leaves are gradually attenuated like the Siberian specimens, and they are not more narrow than in some of Professor Ledebeur's specimens from the Altai. This may be considered the most southern appearance of the plant, none having been found farther north than Pentanguishene, on Lake Huron (Dr Todd). The specimens are from 1½ to 2 feet high, and the stems bear from 10 to 14 flowers. The var. γ is the most northern, and extends from Canada (Mrs Percival and Mr Sheppard) through the whole woody country, and even to the barren regions on the Mackenzie River (Dr Richardson); while Mr Drummond has met with it as far west as Edmonstone House, upon the Saskatchawan. This is usually smaller than our var. β, but it differs in no other respect, save in the leaves being of equal width from the base to the extremity. It is admirably described by Dr Richardson in the second edition of Franklin's First Journey; only that the more copious specimens found during the Second Journey, shew that the stem ought to be described as "ramosus multiflorus," rather than "simplicissimus uniflorus;" a correction, indeed, made by Dr Richardson himself, in a manuscript note before us.

Houstonia ciliolata, Torrey. Banks of the Maitland River, Goderich, U. C.—*H. longifolia*, Willd. Banks of the Maitland River, Goderich, U. C.—*H. patens*, Ell. On mossy grounds in the elevated woods near Pittsburg Penn.

BORAGINEÆ.—*Batschia Gmelini*, Mich. On dry moorland ground, near Brentford, Upper Canada. This is recorded by Pursh and other authors as a native of the southern states.

B. canescens, Mich.—Dry sandy ground, St Clair Bay, Upper Canada.

OROBANCHEÆ.—*Orobanche Americana*, Linn. Woods at Denwiddy, St Clair River, Upper Canada.

LABIATÆ.—*Dracocephalum virginicum*, W. Peninsula of Toronto, Upper Canada. Bordering the swampy grounds on the Peninsula, this plant made a very fine appearance, being covered with its bright purple flowers. This is by no means, however, an abundant plant in Canada, as Niagara is the only station quoted for it; we were not fortunate enough to see it near the Falls, neither on one side nor the other. Its principal habitats in the United States are the mountain-meadows of Virginia and Carolina.

Hyssopus nepetoides, Linn. Fertile places on the banks of the Thames River, New London, Upper Canada,

PRIMULACEÆ.—*Primula mistasinica*, Mx. Moist places on the banks of the Maitland River, Goderich, in abundance.

CHENOPODEÆ.—*Chenopodium fruticosum*, Linn. Salt marshes, Tuckerton, New Jersey. This plant is described by the American authors as the *C. maritimum*, but it is undoubtedly the *C. fruticosum* of Linn.

EUPHORBIACEÆ.—*Euphorbia platyphylla*, Linn. Umbellæ 5-fidæ, radiis 3-fidis, bracteis cordatis foliisque lanceolatis serrulatis subtus pubescentibus appendiculis involucri rotundatis, capsulis verruculosis glabriusculis. *Spr. Syst. Veg.*—*Hab.* Banks of the river St Clair.—This plant has not been before noticed as a native of America, although it is the most abundant plant on the Canada side of the St Clair River for many miles. It always occupies the poorest soil and bleakest situation.

Euphorbia corollata, var. *hirsuta*. Banks of the St Clair River. This variety is found mixed with the common plant, but by no means plentiful. It is readily distinguished by its smaller umbel, and broader and more obtuse hirsute leaves.

Euphorbia polygonifolia, Linn. Dry sands on the banks of Lake Ontario and Lake Huron.

AMENTACEÆ.—*Salix conifera*, Waugh; *S. longirostris*, Mx. Dry sandy grounds, Peninsula of Toronto.

This remarkable species of willow we saw in considerable quantity on the peninsula at Toronto, forming bushes from 5 to 15 feet in height. The branches are generally terminated by silvery, cone-like excrescences, which are entirely the work of insects. The foliage being also of a silvery white, gives to the tree a beautiful appearance. This willow has been mentioned by Pursh and other authors, as indigenous only to the southern parts of the United States; but it may now be added to the Flora of Upper Canada.

ORCHIDEÆ.—*Triphora pendula*, Nutt. On the banks of the Ohio River, near Pittsburg.

This curious plant was growing intermixed with *Corallorhiza odontorhiza*, under the shade of *Kalmia latifolia*, at a considerable elevation above the river.

ASPHODELEÆ.—*Aletris farinosa*, Linn. We met with numerous habitats for this plant in New Jersey and Pennsylvania. We also collected it at Brentford in Upper Canada.

MELANTHACEÆ.—*Zigadenus chloranthus*, Rich.; *Melanthium glaucum*, Nutt.

Dry moorland ground near Brentford, and on the moist banks of the Maitland River, Goderich, both in Upper Canada.

The specimens gathered at Brentford agree with Nuttall's description of *Melanthium glaucum*, and with specimens in his herbarium at the Academy of Natural Sciences, Philadelphia. Its height is about a foot, raceme mostly simple, few-flowered, and flowers perfect. The Goderich specimens varied from 1½ to 3 feet in height, raceme mostly branched, many-flowered, and flowers occasionally unisexual.

Tofieldia glutinosa, Mx. Moist meadows, Brentford; and moist places on the banks of the Maitland River, Goderich.

GRAMINEÆ.—*Andropogon furcatus*, Muhl. Dry moorland ground near Hamilton, Upper Canada.

CYPERACEÆ.—*Rhynchospora capillacea*, Torry. Moist places on the banks of the Maitland River, Goderich.

R. cymosa, W. Quaker-bridge Swamp, New Jersey.

FILICES.—*Woodsia ilvensis*, Brown. Crevices of rocks, Mount Olympus, near Troy, United States.

Pteris atropurpurea, Linn. Crevices of rocks on the banks of the Conodogwinit Creek, Carlisle, Pennsylvania.

Lygodium palmatum, Swartz. Shady swampy ground, New Jersey. No plant seen during the whole range of our researches in America afforded us greater interest, or more pleasure, than this remarkable climbing fern. On the 16th of October 1834 we found it in great beauty in a Kalmia swamp, a few miles west of Camden, in New Jersey, at a time when most other herbaceous plants were killed with frost. It is one of the most delicate looking, and, without exception, the handsomest of the American ferns; the fertile stems were found twining upon the smaller twigs of Kalmias, Andromedas, &c. to the height of 2½ feet, while the surface of the ground was beautifully matted with the barren fronds, which were of a fine lively light-green colour.

Remarks on the question, Does the observation made in Coal-mines of Canada and of Baffin's Bay, of plants analogous to those which live at present in equatorial regions, announce a change in the inclination of the Ecliptic? By M. MARCEL DE SERRES.

IN the coal-mines of Canada and Baffin's Bay vegetable remains have very lately been found, identical with plants which are known to flourish at the present day in intertropical regions; and the conclusion has been drawn, that since these plants require a large quantity of light and an elevated temperature, the inclination of the ecliptic must have undergone important variations. This question, lately brought forward by M. Alphonse de Candolle,* appears to us worthy of being examined with serious attention.

We may commence by remarking, that, in proportion as the phenomena of the ancient world become better known, it appears less necessary to have recourse to astronomical causes to enable us to understand them and explain them. Thus, it is now well proved, that the *soulevemens* which have produced the principal mountain-chains of our continents have no connexion with such causes. In fact, the absence of all direct connexion between the direction of chains of mountains, and the position of the poles and the equator, indicates, with sufficient distinctness, that their formation has not depended at all on the displacement of the axis of the earth. In like manner, our mountains could not have been the result of the shock of a comet, even though we suppose comets to have a solidity which they are far from possessing; for the shock of a body in motion would be much more calculated to produce, in the solid external crust of the earth, inequalities disposed more or less symmetrically round one point, than ridges running parallel to one another over a large extent. The other phenomena of the ancient world seem to be equally conceivable, without our having recourse to cosmological causes which are so much the less admissible as they are beyond the ordinary course of events. In fact, a displacement in the axis of the earth, like a change in the inclination of the ecliptic, could not have taken place without great revolutions in our earth, revolutions which its form and its density, increasing from the surface to the centre, are far from announcing.

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* Bibliotheque Universelle de Geneve, July 1834; and vol. xviii. of Edinburgh New Philosophical Journal, p. 81–102.

Without doubt, the theory of attraction shows that the obliquity of the ecliptic experiences secular variations; but these variations are confined within limits so narrow, that from them no notable changes could result to our climates. Besides, it is not at all in these variations that we can find an explanation of the phenomenon which we are now considering.

To understand it, it seems necessary to direct our attention both to the heat and to the light which the polar regions now receive, and the heat and light which they received in ancient geological periods.

These regions at present enjoy, during six months of the year, a very intense light, while they experience, by turns, during the other six months, the effects of twilight, or those which result from the progressive enfeebling of the light. These regions, then, seem little favoured in respect to the continuity of the action of this fluid, or to the constant and permanent brilliancy of the solar rays. But, in our regions, the time during which the sun shines with all its brilliancy is followed by that during which it is below the horizon, and consequently one part of the earth is seen constantly illuminated during the twenty-four hours which compose the day, owing to the alternations of days and nights. In the polar regions the intensity of the light does not experience any of these alternations during the six months of day; also it is extremely probable that this constant intensity compensates in some measure for what it wants in duration. Hence, may it not be reasonably supposed, that if the polar regions had, at the present day, the same temperature as that which they had in geological epochs, they would still possess plants analogous to those of intertropical countries, the quantity of light which they receive being sufficient to make them grow with vigour? Besides, do we not see at the present time a great number of trees and plants vegetating with vigour in climates, where the light has little brilliancy, on account of the imperfect solution of vesicular vapour? Forests also are generally more numerous and more extensive in the regions of the north, than in countries to the south, where, however, the light is more intense and more brilliant. The coffee-plant of Arabia, where the purity of the sky is almost constant, transplanted to the Antilles, has acquired an increase of size and vigour which it did not possess in its native country. On the other hand, there are a great many plants which do not prosper, except when shaded from a bright light, as under the shelter of the trees of forests; such, for instance, as our *Equiscta*, our *Lycopodia*, and the greater number of our *Ferns*. Finally, are there not some plants which are sufficiently acted on by a bright light, but one of short duration? Can we forget that, on high mountains or in northern regions, vegetation goes to a certain extent through the same circle in three months, which in other places requires more than double the time? On the other hand, in the countries where the light is the most brilliant, there is also a season during which it is often very feeble and nearly absent; it is also the period during which vegetation is almost suspended.

The essential condition, then, which enabled plants analogous to those of intertropical regions, to exist in polar regions, is more connected with the higher temperature which these regions must have enjoyed, than with the quantity of light which they received. For this temperature must have been much more considerable, since it was sufficient for the largest land animals which ever existed, the mastodons, elephants, and rhinoceroses. This high temperature did not depend, as, indeed, is admitted by M. Alphonse de Candolle himself, on a change of the inclination of the ecliptic; but has been due solely to the radiation of the internal heat of the globe, which took place in geological epochs.

The high temperature, which not only the polar regions, but also all other parts of the earth, enjoyed at that period, was, in fact, produced by that which the interior of our planet transmitted, since, in consequence of the cooling of its external crust, the temperature of this surface is now nearly reduced to that imparted by the solar rays. Hence, on the one hand, the very considerable heat of the interior of the globe, at small distances from the surface, and on the other, the slow, but constant diminution of that temperature, enable us to conceive easily how animals and vegetables of which analogous species now live only in the warmest regions of our globe, formerly lived in the polar regions.

In fact, at the epoch when these plants of the ancient world existed, the heat belonging to the globe itself being united to the solar heat, rendered the temperature of different climates much more elevated; but this temperature seems to have diminished more rapidly at the poles than in temperate regions, and more so in the latter than in the hottest regions of the earth. Each of the terrestrial zones, then, has passed through the equatorial temperature after having been subjected to the effects of another still more elevated, and has been reduced to a temperature hardly depending on any other cause than solar heat.

Without doubt the temperature of the same point has varied with extreme slowness, the general phenomena necessarily regulating particular facts. But that does not prevent the places where it has been highest from preserving for a longer time that which belonged to them, which seems to have been the case, with the equatorial regions compared with the polar regions, as with the first of these and the temperate regions.

The facts which we have brought forward in our memoir on Fossil Mastodons, seem to shew, that climates, in establishing themselves, have preserved amongst themselves the same relations as at the present time; and that life, at the surface of the globe, has been disturbed only in a successive and gradual manner.*

The zone in which a particular plant or animal could live must have been continually advancing from the pole to the equator, and would arrive at its present invariable position so much the more speedily in proportion as its position had been near the poles. According to these facts, it would be possible that these polar regions had enjoyed for a shorter time than the equatorial regions, a temperature sufficiently elevated to enable to live and prosper animals and vegetables whose analogies exist only in the warmest regions of the earth. If, then, mastodons, elephants, and rhinoceroses have formerly lived in the polar regions, they could only do so because they found large plants proper to nourish and support them; and both animals and plants could only live, from having the temperature and light which the wants of their existence required. In fact, if light is necessary to vegetables, it is not less so to animals. The latter could change the places where they had just stationed themselves when the circumstances in regard to light were changed. A similar advantage is not possessed by vegetables.

Light undoubtedly produces sensible effects on vegetation; it favours it, and quickens it pretty generally; but, whatever its influence may be, it is much less considerable, and much less absolute than that of temperature. To be convinced of this it appears to be sufficient to cast one's eyes on hot-houses. In fact, the plants of climates where the light is more brilliant and more intense flourish in the most foggy countries, and countries which are in some measure deprived of the influence of direct light.

It would, doubtless, be curious to study the action and influence of the luminous fluid on the plants of the different countries of the globe, but we want precise data to determine the question; we shall do no more than call the attention of the investigators of physical science, and travelling naturalists, to this subject.

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* Mémoire sur les Débris de Mastodontes decouverts en France; inserted in the Mémoires de la Societé des Sciences de Lille, vol. iv. year 1829.

If we consider the very varied forms of corollas, if we consult analogy, the only guide which we can follow in researches of this nature, we should perhaps be led to think that the individual plant, like the individual animal, has not only received from nature the organization the best adapted for fulfilling the nutritive functions in the most complete manner, but also that which could best connect it with the elements which surround it.

If we see that in animals organs are modified according to the places which they inhabit, why should there not exist a perfect harmony between the external form of the vegetable and the larger or smaller quantity of light or heat which it receives?

In order to give a perfect solution of this question, it would be necessary to know the intensity of light in different climates, and to compare the appearance of the plants of one country with that of the plants of another region, enjoying a different degree of heat. The first of these data is entirely awanting. The second can throw some light on the question which occupies us.

First of all, since we cannot conceive the solar light as independent of the heat, although the contrary proposition might be admitted; and, on the other hand, we know that the warmest regions are also the most brilliantly illuminated; these two circumstances united ought, if the principle we have mentioned above is correct, to have modified more or less the vegetables submitted to their influence.

It is then extremely probable, that the external organization of vegetables is constantly in harmony with the larger or smaller quantity of heat and light which they require, in order to arrive at their complete development, that is to say, at the entire termination of their existence. Hence we can well conceive that the organization of the ancient *equiseta*, the *lycopodia*, and the *arborescent ferns*, should be appropriate to the circumstances under the influence of which these plants lived. Thus, an elevated temperature was sufficient for their conditions of existence, and a brilliant light, in all its splendour, was probably not necessary. These circumstances occurred in the polar regions, at the geological epochs during which the ancient plants flourished there; hence the existence of these plants in these regions may be conceived, without our having recourse to changes so considerable as those which would have depended on the variation or the important modification in the inclination of the ecliptic.

The light which the polar regions enjoyed in ancient geological epochs, during at least half the year,—light so much the more intense, because the solar rays then traversed masses of air extremely heated and transparent, in consequence of the complete solution of aqueous vapour,—was doubtless sufficient for the life of these ancient vegetables. Its brilliancy and intensity during this lapse of time, compensated then in some degree for its nearly total absence, and its entire want, at a certain period of the year.

This hypothesis is so much the more admissible, because we are completely ignorant if these arborescent ferns, these equisetaceæ, these lycopodiaceæ, of the coal formation, required a large quantity of light, and because the larger quantity of the species analogous to these last actually live in the most shaded places. Besides, it is possible that it may have been the case with these plants, as it is with many which still flourish, that a too great brilliancy of light may have fatigued instead of being of advantage to them. Such are, for example, nearly all the species of the genus *Mirabilis*, and the *Silene nocturna*, whose flowers expand only after the setting of the sun. Alpine plants, those which live constantly in gloomy shady places, and which, in consequence of this and other circumstances, cannot be raised, and cannot be made to flourish in our gardens, are also a proof of this. There are besides so many degrees in this respect in the vegetable scale, that we may very well admit that the trees and plants may have been able to live, as we have already remarked, in places alternately illuminated by a brilliant light, or nearly totally deprived of its enlivening influence.

In fact, all plants are far from requiring a large quantity of light, and perhaps it is in this respect as in regard to the temperature which is necessary for them. The difficulty, however, does not exist relative to this last influence, since all the facts show that these polar regions had, at the epoch when these arborescent ferns, equisetaceæ, and lycopodiaceæ lived, a temperature equal to that of intertropical countries, and sufficient to enable the large pachydermatous animals, of which we find such numerous remains, to fulfil all their conditions of existence.

Thus the presence, in the polar regions, of fossil plants, of which the analogues live only in the hottest countries of the earth, does not in any degree prove that there has been a change of the inclination of the ecliptic; for these plants could flourish under the influence of the elevated temperature, and of the light which these regions then enjoyed. Besides, we should only admit such a change, which would have caused so many others, with extreme reserve, and if forced by the evidence of facts. Such a necessity not existing, it should be sufficient for us to have been able to conceive these phenomena by the aid of induction and analogy, the only one which is available to us for seizing the facts belonging to periods when there was no human witness.—*L'Institut*, No. 88.

Account of the Founding of a New Grand Observatory for the Russian Empire, by the Imperial Academy of Sciences of St Petersburgh.[*]

It is a truth which can scarcely be denied, that the cultivation of the sciences, both in the department of physics and natural history, is greatly promoted by royal patronage. These sciences imperatively require establishments, the original founding and subsequent maintenance of which far exceeds the abilities of any private individuals. This remark is especially true with regard to Astronomy, the history of which clearly demonstrates the intimate connexion which exists between its advance and the protection of princes; whilst it is further interesting to observe, that monarchs themselves have frequently taken an active share in the extension of this science.

It was when Ptolemy Philadelphus invited wise men from Greece to Alexandria, and with their help formed a school afterwards so celebrated, that in 250 A. C. we see the rise of the first establishment in which the heavens were studied for a long series of years. It was in the portico of the Museum, which was placed close to the palace of the king, in the *Bruchion*, that Eratosthenes fixed his armillary spheres, by which to follow the motions of the stars. A hundred years later, the same apparatus was employed by Hipparchus, the greatest astronomer of antiquity; and by Ptolemy, 150 years after the Christian era. Hipparchus had fixed in the same place his celestial sphere, which exhibited a representation of the stars, arranged according to the discoveries which he had made. Here, too, was found the *Alidade* of this same philosopher, as also the *Astrolabe* of Ptolemy, and his parallel rulers. After a flourishing existence of 400 years, in which the basis of practical astronomy was established, the Observatory of Alexandria began to decline, and it is probable it was entirely destroyed at the time that the Arabian conquest overthrew all the scientific establishments of that country.

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[*] Saint-Petersbourg. From the Press of the Imperial Academy of Sciences.

If the period which succeeded, of nearly a thousand years duration, until the revival of letters, was truly a dark age for the most of the sciences, this is, at the same time, less true of astronomy than of any other. The Arabian caliphs and the Mongul khans vied with each other in their application to the study of this science, and in endeavouring to promote it by the establishment of observatories. The caliph Almamoum founded one observatory at Damas and another at Bagdad; and, at a later period, there was another established at Bagdad by the Egyptian caliph Hakem. At the overthrow of the caliphat, in the thirteenth century, astronomy found protection with the victorious Monguls. Holagu-Ilekan, the grandson of Gengiskan, the conqueror of Mostazem, the last of the Abassides, was a zealous patron of this science. Nassireddin de Thous, the greatest astronomer of his time, then flourished in the East. He had been banished the court of the caliph Mostazem by a low cabal. The brother of Holagu, and his predecessor in the khanate, Mangu by name, had previously declared his intention of founding an observatory, and had instructed his brother Holagu, who acted as his general against the caliphate, to send him the illustrious astronomer, as soon as he had made himself master of Irak. Mangu did not survive to see the accomplishment of this event; but as soon as Holagu had terminated his campaigns, in which he penetrated as far as Egypt, he announced himself the Protector of Science. Nassireddin resorted to his court, and the strictest friendship was forthwith established between the Khan and Kodjah. Immediately after the capture of Bagdad, in 1258, the khan gave orders to the treasurer to pay over to Nassireddin whatever sums he might demand for scientific objects; and the following year the foundation of the observatory in the capital, Meragah, was laid, about seventy miles from Tauris. The building was placed upon a hill in the neighbourhood of the town, and there Nassireddin made observations to a very advanced age; and the result of these labours is what is now known under the name of the Tables of Ilekan, which for a long time have been the most celebrated of the East. A description of this observatory of Meragah still exists, which was drawn up by Mujawad de Damas, the friend and assistant of Nassireddin; from which it appears that the establishment possessed four instruments of Grecian workmanship, and five of Arabian invention, all of such dimensions as to require very substantial buildings for their accommodation.

Previous to the revival of astronomy in Europe, it flourished amongst the Usbek Tartars, whose prince, Ulugh-Bey, grandson of Tamerlan, was beyond comparison the greatest astronomer of the Middle Ages. Ulugh-Bey reigned at the commencement of the fifteenth century, for forty years, and collected in Samarkand, his capital, the most able astronomers of Asia,—though, more learned than any of them, they acted merely as his assistants in his observatory. His instruments were magnificent, and of a prodigious size. Amongst others he had a gnomon 180 feet high, by means of which was determined, by the shadows, the latitude of Samarkand, and the obliquity of the ecliptic. The tables of Ulugh-Bey, and his catalogue of fixed stars, are very properly considered as the principal work on astronomy in the Middle Ages, and imply the taking of observations which surpass in accuracy all that had previously been made.

It is to these princes that astronomy owes its preservation of the knowledge acquired by the Greeks, not less than its extension, and its conveyance into Europe. As early as the thirteenth century, the Emperor Frederick the Second caused the works of Ptolemy to be translated from Arabian into Greek, and Alphonso, King of Castile, introduced the construction of new astronomical tables. But it was in the sixteenth century, that a new era for astronomy, as a science of observation, was opened up, under the auspices of the Landgrave of Hesse, William IV., who enriched the science by a long series of observations; and also procured the protection of Frederick II. of Denmark to Tycho Brachè, the greatest astronomer of his time. The King of Denmark gave the island of Hveen, in the Sound, to this astronomer, for the purpose of there establishing the first observatory of modern times, to which he gave the name of Uranienbourg, all the cost and expense of which was defrayed by the prince. The instruments necessary for this establishment were constructed in a workshop appropriated to the purpose, in which the first German tradesmen worked under the eye of Tycho. He began his observations there in 1577, with twenty-eight different instruments, and, with the help of many assistants, continued them till 1597. This invaluable treasure of observations became, in the hands of his scholar and friend Kepler, the instrument by which were effected the grand discoveries concerning the true forms of the planetary orbits, and concerning the laws of the movements of these planets in their orbits,—discoveries which have immortalized the name of Kepler, and which soon after gave birth, the offspring of Newton's genius, to the whole of physical astronomy. After the death o King Frederick, the Emperor Rudolph became the patron o Tycho, and invited him to Prague, where he was associated witl Kepler.

The extraordinary progress which astronomy made in the 17th century, principally by the discovery of the telescope, is well known; for by it, new means were acquired for revealing to the inhabitants of the earth the wonders of the heavens. Many observatories were then constructed, under the patronage of enlightened princes, at Copenhagen, Paris, Greenwich, &c.; and in the following century, others arose at Manheim, Palermo, Stockholm, and in other places. It would here occupy too much space to relate all that has been done by princes, during this period, for the advancement of astronomy. None can be ignorant how much we are indebted for our knowledge of the heavens to George III. of England, who judiciously supplied to the greatest observer who has yet appeared, viz. to Herschel, the means of entirely devoting himself to his favourite science. The king was himself an amateur, and had at Richmond a beautiful observatory, where, in 1769, he observed the transit of Venus across the sun. Nearer the present time has been founded the Seeberg observatory, near Gotha, under the protection of Duke Ernest. This observatory, raised under the direction of De Zach, is the first on the continent which has been constructed with a complete knowledge of the necessary apparatus for an establishment of the kind; and it has had a great influence upon all the observatories which have subsequently been built, serving more or less as a model to them all, such as those of Gottingen, Dorpat, Munich, &c. The Duke Ernest was not only a patron of the science, but he deserves to be ranked amongst astronomers, for he bore his share in the observations, and even in many of the deep and extensive calculations. It is from the observatory of Seeberg that for a long time has issued the journal known under the name of "*Correspondance Nouvelle*," and published by De Zach and Lindenau, and which has greatly contributed to the advancement of the science throughout the whole of Germany. What the reigning monarch of Prussia has done for astronomy is well known, by his founding the observatory of Königsberg, by his establishing a new observatory at Berlin, as also by attracting to his dominions two men whose names are inseparably associated with the history of the science, viz. Messrs Bessel and Encke. The enlightened interest which Frederick, King of Denmark, takes in astronomy, is likewise well known, as also the protection and consideration which he confers upon artists, and the methods by which he stimulates the zeal of the astronomers of his own kingdom and of other countries, by the deep interest which he takes in their labours. These latter facts are too notorious to require more to be said concerning them.

Turn we, then, now to what has been done in our own country, Russia, in behalf of astronomy. It was when Peter the Great founded the Academy of Sciences, that Russia witnessed the rise of its first permanent observatory. This establishment, at a later period, was endowed by Catherine II. with some very valuable instruments, such as a mural quadrant of Bird's, a reflecting telescope twenty feet long, and several others. By order of this princess, the transit of Venus, in 1769, was observed by twelve native and foreign astronomers, at places the most widely separated in the empire; and on this occasion, as on more recent ones, the journeys of the academicians contributed much to the advance of the geography of the empire. Russia reckons many illustrious sons among the astronomers of the present century, such as Delisle, Lexell, Roumowsky, Inochodzoff, and Schubert; the last of these, especially, may be considered as one of the first theoretical astronomers of our day.

A new era of practical astronomy commenced in Russia with the glorious reign of Alexander. It was then that the observatories of Dorpat and of Abo were founded, and supplied with instruments inferior to none even of the richest establishments in Europe. It was for the observatory of Dorpat that the grand telescope of Fraunhofer was constructed,—the most famous work of this incomparable artist, and one of the most perfect instruments which the history of astronomy has recorded. In the same reign, the construction of a third observatory, that of Nicolajef, was commenced, upon the Black Sea. This enterprize was particularly encouraged by Admiral Greig, well known for his extensive acquirements in practical astronomy, and who endeavoured thereby to procure for the fleet under his orders a suitable and scientific station. The Emperor Nicholas, animated with the same views as his predecessor, has equally favoured the progress of astronomy throughout the empire. By his liberality, he has communicated a new degree of energy to the observatory of Dorpat. The University of Abo, now removed to Helsingfors, is indebted to him for the founding of a new observatory, which, just finished, may be considered as the best constructed in Europe, and which, doubtless, will yield much rich fruit to science, under the indefatigable activity of the learned Argelander. Under the auspices of Nicholas, an observatory has also been founded in Moscow, our ancient capital, and another is in progress at Kasan, which will be inferior to none of its predecessors; and speedily Kiew also, this new focus of instruction, will reckon an observatory among the number of its scientific establishments.

But this kind zeal of the emperor has carried him still further. He has just given instructions that, to the present astronomical establishments shall be added a central and chief observatory, situated in the neighbourhood of the capital of the empire, which shall be the property of the Academy, as the leading scientific body, and which shall be constructed of the largest dimensions, and be in every way suited to meet the most recent exigencies of the science.

Is it demanded, What is the use of so many observatories? The answer is direct and easy. The frontiers of Russia are so extended, that the relative position of the more distant parts of its territory can be determined only by means which are purely astronomical. Thus, then, even in a geographical point of view, astronomy is required for Russia. But, besides, it is not easy to tell its value, at a time when the maritime power of Russia is assuming its proper level. And, besides, how important is astronomy merely as a science! How inexhaustible the field which the science presents for observation! Every thing is changing,—all is in motion in this universe. Armed with his telescope, the astronomer watches the stars at immense distances during the silence of the night; he compares the new results he obtains with older ones, and thence infers the alterations which are taking place in their movements; alterations, some of which are affected in short periods, and others in periods of such vast duration, that we are even now just beginning to recognize the traces of them. How then can a single man, or a small number, ever embrace the whole field of research, even had they Herschel's means of observation, his genius, his perseverance, and his protracted days. It is only by the united uninterrupted efforts of a large number of individuals, that posterity will obtain those results which science promises to supply.

The Academy of Sciences were fully aware of the fact that its Observatory no longer fulfilled the demands of the astronomy of the present day. The instruments, though excellent for the time at which they were procured, were now, so to speak, superannuated, and their position, in an elevated tower, in the midst of a populous city, was any thing but suitable. The means which were employed to remedy its deficiencies had, and could, but partially succeed. In a word, the capital of the empire still required an astronomical institution worthy of itself. For some time past the Academy had not failed to give expression to those views of the matter which was incumbent upon it, and was engaged about the plan of a new observatory. The Emperor, when, in 1831, he improved the position of the Observatory of Dorpat, announced his intention to confer upon the Academy of Petersburg, the means of making observations more in accordance with the present state of the science; and the height of Pulkowa was then indicated as the most favourable spot for the site of an observatory, in the neighbourhood of the metropolis. On the 26th of October 1833, government made a communication to S. E. M. d'Ouwaroff, Minister of Public Instruction, and President of the Academy, requesting him to transmit a plan for the observatory in question. This request could immediately be complied with, inasmuch as the Academy possessed a finished plan of M. Parrot's, one of its members, together with an estimate of the expense of the building, and a list of the instruments necessary for the establishment, and their price. According to these data, the sum necessary for the building amounted to upwards of L. 66,000 (1,600,000 francs), and that required for the instruments to about L. 26,000 (630,000 francs). On the 28th of the same month, the Emperor authorized the Academy to give orders for the instruments, and put L.20,000 at the disposal of the Minister of Public Instruction, that the building might begin in the spring of 1834, on that site which the Academy had selected as the most convenient.

For the purpose of executing the enlightened views of the Emperor, the minister appointed a commission from the members of the Academy, viz. Winievski, Fuss, Parrot, and Struve, over which Admiral Greig, who is an honorary member of the Academy, was to preside. The commission assembled in the month of November, and agreed to consider the previous labours of the Academy in the matter to be merely of a preparatory character. The object being to establish in the neighbourhood of St Petersburg, an observatory which might prove a centre to the astronomical labours carried on throughout the Russian empire, the commission examined with the greatest care, what was desiderated for an establishment considered in this point of view, so that it might give directions for the construction of the building, and the purchase of instruments, for this specific purpose. Accordingly, it was occupied, in the first place, with the regulations of the observatory, the design of which were to refer to the three following purposes:

1st, To supply the means of making regularly, and as perfectly as possible, the observations required for the wants of astronomy as a science.

2d, To make such relative observations as might be required for the accomplishment of geographical enterprises, whether in the interior of the empire, or for foreign voyages. And,

3d, To do all that was possible for improving practical astronomy, in its applications both to geography and navigation.

The commission next proceeded to determine what apparatus was necessary for the purposes of the observatory, and the formal attendance which would be required for carrying on the observations; and, finally, the accommodation the building must supply, in relation both to the science and economy. Moreover, having made a fresh examination of the neighbourhood of St Petersburg, they were quite satisfied that the height of Pulkowa, already pointed out by the Emperor himself, was the most eligible situation. In December the Emperor commanded the plans of the commission to be printed, and charged MM. Brüloff and Thon, architects, members of the Academy of Beaux-Arts, each to present a plan of the observatory, agreeably to the instructions of the commission. The two plans were submitted to the commission at the end of February 1834, when it determined to accept that of M. Brüloff, and requested him to subject it to some further revisal, that it might in every respect answer their views. Finally, this plan having been presented by M. Struve to the Emperor, was approved by him in all particulars, and M. Brüloff was charged with its execution. The propositions of the commission, relative to the purchase of the instruments, were also approved of, and the ground intended for the establishment, on the heights of Pulkowa, was made over to the Academy. The Emperor then verbally charged Mr Struve, to visit, during the approaching summer, Munich, Hamburgh, Altona, to have formal communication with the celebrated artists of these towns, respecting the instruments; he was even authorized to extend his journey to Paris, London, or wherever he chose, the intention of the Emperor being, that, at its opening, the observatory should be provided with the most perfect instruments which the mechanician or the optician could supply. According to the estimates made, the sum requisite for this object amounted to about L.37,000 (890,000 francs). The cost of the building could not be accurately known till the work was finished; but it might be presumed that the whole establishment, when completed, including the building and the instruments, would cost not much less than L.200,000 (4,600,000 francs), without allowing any value, though in itself considerable, for the ground presented by the Emperor.

The height of Pulkowa, which forms a part of the imperial domain of Zarzkoje-Selo, is situated about twelve miles to the south of the harbour of Petersburg. The great road which leads to Zarzkoje-Selo, runs in a straight line quite to the foot of the hill, which rises rapidly, at this point, about 200 feet above the road, and then inclines toward the left. The surface of the elevation is composed of a hard clay, which forms a base extremely favourable to the perfect solidity of the foundations of the building, a condition which is above every requisite in the site of an observatory. The sides of the hill are covered with trees, and on the summits the soil is fertile, and laid out in fields and gardens, and planted with a pleasure grove. Up to the present time the spot has been rented by farmers, who will be amply remunerated for yielding it up to its new proprietors. The space allowed for the observatory occupies exactly 75 acres; its largest extent, from north to south is 2205 English feet, and its smaller from east to west 1582 feet. The observatory will be placed near the middle of this space, in such a way that the centre of the building will be exactly upon the prolongation of the line of the great road of Petersburg, and it will accurately front in this direction. The circumstance that the road winds to the east at the foot of the hill, and the height of the hill itself above the road, give assurance that the observatory will be removed from every thing like concussion, and also from the dust which this proximity might suggest as a cause of apprehension. This position also gives the establishment the advantage of being elevated above the vapours which prevail in these low parts, and also the benefit of a purer air. The distance at which the capital stands to the north, puts it equally out of the influence of smoke from the city. In a word, it is difficult to conceive a locality altogether more favourable for an observatory. The view is quite free in all directions, and the prospect which is presented is alike charming and varied: On the north is seen the capital, on the south the castle and domain of Zarzkoje-Selo and of Paulowsk, which is at no great distance; to the west the Gulf of Finland, with Kronstadt, Strelna, Oranienbaum, and, finally, on the east, the rocky mountains of Finland.

The observatory will consist of two parts, the building intended for scientific labour, or the observatory in the strict sense of the word, and other buildings intended for dwelling-houses and their dependencies. The whole will occupy an extent of space 840 English feet long, in the direction from east to west. The observatory properly so called, occupies the middle portion, and forms a cross, extending 220 feet from east to west, and 175 feet from north to south. In the great body of the building, there will be, in the ground floor, a large hall, intended to be a repository for instruments, which are not in daily use, and for the reception of visitors, also two other halls, with a southern exposure, and each with two perpendicular slits in them, also a chamber for the regulating of pendulums, with an apparatus, calculated to produce very great differences of temperature; also the hall for the Directors, the library, a lecture-room, and five small apartments, in which the observers may take repose, or warm themselves, or work, and which are placed commodiously near the places where the observations are taken. Upon the building three towers will rise, having revolving roofs; that of the middle having a diameter of 32 feet, and the two others of 20 feet each, intended to receive the instruments, by means of which all the points of the celestial vault may be observed. On the east and west of the observatory properly so called, two corridors are placed, 91 feet long, which will establish a communication with the dwelling-houses, and which are thus placed on purpose, at a great distance from the observatory. Four buildings are intended for dwelling-houses, which are detached, and separated from their dependencies, where, among other establishments, there will be a small forge. We may consider also as belonging to the observatory, or chief building, four tents, distributed symmetrically round it, two of which are intended to receive those who are on the outlook for comets, and the other two those instruments of geodætical astronomy, which are of a great weight. The corridors, which we have mentioned above, may also supply situations very fit for the employment of small instruments in the direction of the meridian. The officers of the observatory, consisting of a director, of four astronomers, two inspectors, a mechanician, secretary, and domestics, will find in the dwelling-house large and most comfortable habitations. The platform of the hill will supply gardens for the several families which reside in them, and beyond those extensive pleasure-grounds.

The time is not yet arrived when we might enter upon the detail concerning the various instruments which will be employed in the observatory. The Commission has already opened up communications with many artists, but waits the return of M. Struve before coming to any final conclusion. The celebrated optical manufactory of Munich, which was formerly under the direction of Fraunhofer, and now of Messrs d'Utzschnieder and Mertz, has promised to supply to the Petersburgh Observatory a refracting telescope, larger than any they have previously made, and as perfect as those which have been manufactured by Fraunhofer. The Commission hopes, that, by profiting by the experience of the last twenty years, and retaining that which is known to be good, and improving whatever may require it; in short, by introducing improvement in various particulars, they may also procure for their other instruments an uncommon degree of perfection.

The Commission will continue its labours until the observatory be completely finished. Its watchful care will be directed to ascertain that the construction of the building answers, in every respect, to its scientific destination, whilst that department which is purely architectural, will be under the inspection of Mr Brüloff. The Commission itself will especially give directions concerning the revolving roof-work of the towers, to which is to be applied a mechanism, which has already been employed for nine years with complete success, for the revolving cupolas of the Observatory of Dorpat, by two of the members of the Commission, viz. by Messrs Parrot and Struve. The Commission will continue in permanent communication with the artists who undertake the construction of the instruments, till the time they are received into the Institution.

As the construction of the observatory is subjected to certain particular regulations, and the making of the instruments requires much time, it can scarcely be expected that the establishment will be opened sooner than the year 1838, or the commencement of 1839. So soon as that event happens, the labours of the Commission will be considered as terminated, and the Academy of Sciences will entrust the superintendence of the observatory to a Director, who, as already stated, will be assisted in his scientific pursuits by four astronomers; the direction and distribution of whose labours will be always regulated by the Director, so that they may always be conducted upon a fixed and uniform plan. At the expiration of every year, the Director will present to the Academy the journal of the observations which have been made in the establishment, and the Academy will undertake to publish them, during the course of the following year, with a Latin text, under the title of *Annales Speculæ Imperialis in Academiâ Cæsareâ Petropolitanâ.* The Emperor has granted, for the maintenance of the observatory, an annual revenue of more than L. 9000 (218,000 francs), to commence so soon as the building is finished.

And thus the Academy of St Petersburg beholds one of its earliest wishes realized, in a manner which far surpasses its most sanguine expectations. To the scientific establishment which it already possesses is now to be added an observatory, the founding of which is not the least remarkable event in the records of astronomy.

Account of some remarkable Hailstones which fell at Padua, on the 26th of August 1834. By D. L. Cosari [*].

After some descriptive details of the terrible hurricane which took place on the 26th of August at Padua, the author proceeds to the examination of the minute structure and form of the large hailstones which fell during the storm. All his remarks are taken from the observations he himself made on the spot to which he fled for refuge, and which were repeated as soon as he could without danger expose himself, and pick up particular hailstones from the mass under his feet. He, however, adds, that his own observations, in every particular, coincided with those which were made by other individuals in all parts of the city.

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[*] Ann. delle Scienze del Regno Lomb. Veneto. Nov. et Dec. 1834.

It has been generally remarked, says the learned Italian, that the hailstones were sometimes of a globular, and sometimes of a flat form, and rough on their surface, with points and crystals. For the sake of precision, I shall divide the remarks I am about to make into two classes; the former will comprehend those on the hailstones which were of a flat and irregular shape, and the latter upon the spherical ones.

Hailstones of a Flattened and Irregular Shape.

(1.) Some of these fragments of ice presented the appearance of an angular plate, of about an inch in thickness. Across one of these surfaces, which was nearly flat and transparent, might be perceived fine plates, some of which were rectilinear, and others curved, opaque and whitish, which alternated with other plates of transparent ice, which were not concentric, but nearly parallel to the largest of the lateral faces of the plate. Adhering to the opposite surface, there was also seen crystals of very pure and transparent ice, quite distinct from each other, but interlaced among themselves. They were inclined on the surface, and stood at an angle of about 45°; the length of these crystals was nearly an inch and a half, and their form that of a four sided prism, one side of which was very small in comparison of the other three. Each prism was terminated by a pyramid which had also four sides. The plates presented the appearance of a crystallized sediment. The greatest distance comprehended between two of the angles which they formed, varied from four to eight inches.

(2.) It was also observed, that the irregular pieces of ice, which were of a flattened and somewhat double-convex shape, presented a rough surface, with an angular and irregular outline, and mixed with rudimental four-sided prismatic crystals. There were also a great number of a circular and elliptical form. They presented circles or ellipses formed of concentric layers of ice of an opaque whiteness, alternating with transparent layers, in the centre of which there was a white or opaque nucleus. The diameter of these plates of ice was from 1½ inch to 4 inches.

(3.) There were also observed, other pieces of transparent ice, whose surface was rough, and the edge of which, thicker than the rest, formed a kind of border. The edge alone presented striæ, produced from three to five alternate plates of ice, sometimes whitish opaque, and sometimes transparent. The centre of the exterior portion was a whitish circle, which appeared the nucleus. The transparent centres of these pieces were sometimes perforated, so as to present, after partial melting, the appearance of a ring of ice, the diameter of which varied from 1½ inch to 3 inches. Many individuals were struck with the appearance which we have just mentioned.

Hailstones of a Spherical Form.

(4.) Some of these fragments presented a covering of transparent ice, with an opaque and whitish interior. When these were cut into two parts, there was found underneath the covering, whitish opaque layers, alternating with transparent ones; either disposed irregularly, or following the direction of radii. The transparent layers were much smaller than the white opaque ones; so much so, that at first view they seemed to be entirely whitish, and without striæ. The diameter of this variety of fragments was from 1 to 3 inches.

(5.) The fifth species of fragments which were noticed, approached very nearly to the spherical form. Externally they were smooth, and composed of alternating layers of opaque white and transparent ice, with a whitish nucleus.

It was remarked that the number of layers in the fragments whose diameter did not exceed an inch and a half, was never greater than nine, nor less than seven, without including the nucleus. The exterior layer was always transparent, and thicker than the interior transparent layers.

The number of striæ in the fragments whose diameter was less than an inch, was from three to five, without including the nucleus. The diameter of all the fragments of this class was from nine lines to nearly four inches.

(6.) There was another class of fragments resembling those we have last described, as it regards the size and the interior structure, but whose surface was covered with transparent crystals, in the form of hexahedrons, as was evident by one of their sides. Many among them were incomplete; whilst some others presented the section of a pyramid, with a quadrangular base. The length of these crystals did not exceed two lines.

(7.) Other hailstones were smooth on the surface, transparent, and enclosed a mass of an opaque white colour, not striated, and without any distinct nucleus. The diameter of these did not exceed an inch.

(8.) There was yet another class, the surface of which was smooth and transparent, and when cut did not present any of those lines which were so commonly noticed. They, however, contained bubbles either of common air, or of some other gas, around which a fine whitish net-work might be perceived, but so minute that at first view the whole appeared to be transparent. These pieces of ice, which were about an inch in diameter, were exceedingly abundant, as, in truth, were all the other varieties.

The opaque layers of ice were of the same consistence with the transparent ones, for neither of them could be penetrated by a fine and sharp metallic point. In many of the hailstones the nucleus was softish, and in many others it was as hard as the layers which surrounded it. It was generally observed, that, in the small striated hailstones, the whitish nucleus was harder than in those of larger dimensions.

The size of the different striæ in the hailstones described in the paragraphs marked 5 and 6, were always proportioned to the diameter of the hailstones to which they belonged. They were irregular also, when the surface of the hailstone was irregular. The fragment described under the paragraph 2, belonged to this category.

The hailstones described in the paragraphs marked 5, 6, and 8, when plunged into water, rose to the surface, though they did not rise high above its surface; whilst the hailstones described in the paragraphs marked 4 and 7, on the contrary, rose up high above the level of the water,—a fact which proves that the former, composed chiefly of whitish opaque ice, was much more dense than the latter.

Among all the hailstones which have been examined during and after their descent, not one has been found which gave the idea that it was formed by an agglomeration of several hailstones. It is true there were many of a very irregular form, but even these were composed of a single system of particular and distinct striæ.

Several persons thought that they observed stony or metallic substances descending at the same time with the hail, and there were presented to the author as such, fragments of sulphate of iron, and carbonate of lime, and furnace-made brick. But the manner in which these fragments were procured, did not leave him free from doubt as to their origin, and gave ground for the suggestion that their descent might be owing to the effect which the hail had in detaching from the walls and roofs on which it beat, those fragments which were thus collected.

In the interior of a great number of the hailstones, there was noticed a heterogeneous and sandy matter, which was there disposed, in a manner more or less regular, round the centre. Unfortunately, none of the individuals who particularly observed this, thought of examining or preserving any of it. The author himself remarked only two hailstones which appeared to enclose any foreign matter. In the one, these substances were exactly in the centre, and in minute quantity. In the other, the nucleus was of an ash-colour, and about the third of an inch in diameter. On removing the external layers from the hailstone, to the extent that the nucleus was only surrounded by pure ice, he obtained a residue of a light greyish powder, in so small a quantity that it scarcely stained the linen-cloth he employed to collect it. He subjected this powder to the examination of a strong magnifying-glass, and he distinguished that some grains were larger than the rest, though they had the same clear yellow colour, and the same resemblance to sulphate of iron. Some of the grains, which were of a blackish-grey colour, were attracted with the greatest facility by the loadstone. The very minute dimensions, and the extremely small quantity of this dust, which M. Cosari had procured, led him to abandon the determination he had at once formed, of submitting it to chemical analysis. It, however, appears to him that, from the foregoing statements, it may be concluded that this dust was a combination of iron or nickel, with some other substance not ascertained.

The whole of the observations which have been made upon the extraordinary storm which befel the town of Padua, clearly demonstrates the insufficiency of all the physical theories hitherto proposed for the explanation of this class of phenomena; and it is only by future observations that we can hope to dispel that obscurity which still envelopes the whole of this class of subjects.

Account of an Attempt to ascend Chimborazo, made on the 16th December 1831. By J. B. Boussingault. In a letter addressed to Alexander Von Humboldt. *

After ten years' uninterrupted labour, I realized the project of my youth which led me to the New World. The height of the barometer at the level of the sea between the tropics, was ascertained in the harbour of Guayra, the geographical positions of the capitals of Venezuela and New Granada, were determined, and numerous measurements had afforded us information in regard to the heights of the Cordilleras. I had collected the most accurate information regarding the geological positions of the gold and platinum of Antioquia and Choco, I had established my laboratory successively on the various craters of the equatorial regions, and had been so fortunate as to carry my investigation of the decrease of temperature in the Intertropical Andes, to the immense height of 5500 metres.

I was at Rio-Bamba recruiting myself, after my first expeditions to Cotopaxi and Tunguragua. I wished to give up my investigations, and, as it were, satiate myself with the contemplation of those majestic glories which had so often inspired me with a zeal for science, and to which I was now about to bid an eternal farewell.

Rio-Bamba is perhaps the most extraordinary diorama in the world. The town presents no objects of interest; it is placed on one of those arid *plateaus* which are so common in the Andes, and all of which, on account of their great elevation, have a peculiarly wintery aspect, that awakens in the traveller a certain feeling of sadness, owing doubtless to the circumstance that, in order to reach them, the most picturesque scenes are traversed, and also to regret at exchanging the climate of the tropics for that of the north.

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* Metre is equal to 3,28 feet Engl.

From my dwelling I looked out on Capac-uru, Tunguragua, Cubillé, Carguairazo, and finally towards the north on Chimborazo; I saw also several other celebrated mountains of the Paramos, and which, though not covered with perpetual snow, are not the less interesting to the geologist.

The mighty amphitheatre of snow which bounds on all sides the horizon of Rio-Bamba, presents incessantly subjects for observations of the most varied description. It is singular to watch the varying aspect of these glaciers at different hours of the day, to see how their apparent height is, by the action of atmospheric refraction, perpetually changing. With what interest does not one contemplate the display, in so small a space, of all the chief meteorological phenomena. Here one of those immense broad clouds, which were by Saussure so well named parasitical clouds (*Schmarotzerwolken*), begins to attach itself to the middle portion of a trachyte cone; it adheres firmly to it, and is not at all affected by the wind, however strong it may be. Soon a flash of lightning darts from the centre of this mass of vapour; hail, mixed with rain, descends on the foot of the mountain, while its snowy summit, which the storm cannot reach, is brilliantly illuminated by the sun. At a greater distance, rises a summit of bright shining ice; its outline is delineated with sharpness on the blue heaven, and all the peculiarities of its form are distinctly visible. The atmosphere is of remarkable purity, nevertheless the snowy summit becomes covered by a cloud. The cloud seems to come from the interior of the mountain,—one could almost believe he sees smoke ascending; a little later this cloud is nothing more than a thin vapour, and then is entirely dissipated. But not long afterwards it again appears, merely once more to vanish. This intermitting formation of clouds is a very frequent occurrence on the summits of snowy mountains. It is to be seen more particularly in fine weather, and always some hours after the culmination of the sun. Under such circumstances, the glaciers may be compared to condensers which project upwards into higher regions of the atmosphere, in order by cooling to dry the atmosphere, and restore to the surface of the earth the water which the atmosphere contained in the form of vapour.

The plateaus, surrounded by glaciers, present a melancholy appearance, when a continued wind carries thither moist air from warm regions. The mountains are obscured from view; and a bank of clouds, which seems to touch the earth, covers the horizon. The day is cold and moist, for this mass of vapour is almost impenetrable to the rays of the sun. A long twilight takes place, the only one known under the tropics; for in the equatorial zone the night follows the day so suddenly, that one would be inclined to believe that the sun was extinguished at its setting.

I could not better terminate my investigations on the trachyte of the Cordilleras than by a particular examination of Chimborazo. To pursue this object, it was only necessary, it is true, to approach the foot of the mountain; but the hope of obtaining the mean temperature of a very high station induced me to attempt the ascent, and, though my hope was not fulfilled, yet I trust the expedition will not be without its use for science. I mention here the causes which led me to ascend Chimborazo, because I must severely blame the perilous attempt when it is not undertaken in the cause of science. Thus to me it appears that Saussure is the only one who has reached the summit of Mont Blanc, notwithstanding that since the time of that distinguished traveller the mountain has been more than once ascended. To his imitators we have no thanks to render, for they have taught us nothing worth the risk of such a journey.

My friend Colonel Hall, who had already accompanied me to Antisana and Cotapaxi, wished to join me on my present journey, in order to complete the extensive information he possessed regarding the Province of Quito, and to prosecute his investigations on the geography of plants.

From Rio-Bamba are seen two extremities of Chimborazo of very different inclinations. One, towards Arenal, is very steep, and presents pinnacles of trachyte projecting from the ice. The other, which is directed to the district called Chillapullu, not far from Mocha, is, on the contrary, by no means steep, but of considerable extent. After we had examined the mountain from all sides, we determined to attempt the ascent by the last mentioned acclivity.

On the 14th December 1831, we took up our quarters for the night at the farm of Chimborazo; we were so fortunate as to find some straw for a bed, and a sheep's skin as a protection against the cold. The farm stands at a height of 3800 metres; the nights are cool, and the place rendered more disagreeable by the scarcity of wood. We were already in the region of the grasses (*pajonales*), which is traversed before the limit of perpetual snow is reached. Here trees cease.

On the 15th, at seven o'clock A. M., we commenced our journey, guided by an Indian from the farm. In general the Indians of the Plateaus are very bad guides, for, as they seldom ascend beyond the snow limit, they know very imperfectly the routes which lead to the ridges of the glaciers. We ascended along a stream which, enclosed between two walls of trachyte, receives its water from the glacier. But we soon left this ravine in order to direct our steps towards Mocha, along the foot of Chimborazo. We ascended very gradually. Our mules had a fatiguing difficult march amongst the heaps of blocks at the foot of the mountain. The acclivity became very steep, the ground insecure, and the mules paused for some time at almost every step; they no longer obeyed the spur; they breathed quicker and kicked. We were then exactly at the height of Mont Blanc, for the barometer indicated a height of 4808 above the sea.*

After we had covered our faces with a mask of gauze to protect us from the misfortunes we suffered in Antisana, we clambered up a ridge which terminated in a very high point of the glacier. It was mid-day. We ascended slowly, and in proportion as we got deeper in the snow, the difficulty of breathing while walking became more sensible. We recovered ourselves easily, however, by standing still every eight or ten paces, but never sitting down. I think that I have remarked that at the same height the breathing is more impeded on the snow than on the rocks. I shall afterwards endeavour to explain this.

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* Mont Blanc is 15,666 English feet high.

We soon reached a black rock, which was elevated above the ridge upon which we advanced. We still continued to ascend for a time, but not without great fatigue, caused by the slight consistence of the snow, which continually gave way under us, and in which we often sank to the knees. Notwithstanding all our exertions, we were soon convinced of the impossibility of advancing farther; for beyond the black rock the moveable mass had a depth of more than four feet. We placed ourselves on a block of trachyte, which resembled an island in the midst of a sea of snow. We were at a height of 5115 metres. The temperature of the air was 2°.9 cent. It was half past twelve o'clock. After all our fatigues, we had only attained a height of 307 metres above where we commenced our first journey. At this point I filled a flask with snow, in order to examine chemically the air contained in its pores, and of this I shall afterwards speak.

In a few minutes we had returned to the place where we had left our mules. I employed a few minutes in examining geologically this part of the mountain, and collecting a series of the rocks. At half-past three o'clock we commenced our journey downward, and arrived at our farm at six o'clock.

The weather had been delightful. Chimborazo had never appeared to us so majestic; and after our fruitless journey we could not contemplate it without regret. We now resolved to attempt the ascent from the steepest side, that is the side towards Arenal. We knew that it was on that side that Humboldt had ascended. The limit had been pointed out to us from Rio-Bamba to which he had reached; but we found it impossible to obtain exact information as to the route he had pursued. The Indians who had accompanied this intrepid traveller were dead.

Next morning at seven o'clock we proceeded towards Arenal. The sky was remarkably clear. To the east we saw the celebrated volcano of Sangay, which lies in the province of Macas, and which La Condamine had seen in a state of constant eruption. As we advanced, the ground became distinctly more elevated. In general, the trachytic plateaus at the foot of the isolated peaks, with which the Andes are as it were sown, rise gradually to the foot of these mountains. Numerous and deep clefts, which furrow these plateaus, seem all to be directed to one common point, and they become narrow in proportion as they are more distant from this centre. One cannot describe them better than by comparing them to a cracked glass (*verre etoilé*).

At two o'clock we halted, in order to breakfast, under the shadow of an enormous block of trachyte, to which we gave the name of *Pedro del Almuerzo.* Here I made a barometrical observation, as I hoped to make another at four o'clock, and so learn the daily barometrical variation. This station is at a height of 4325 metres. We passed the snow limit, sitting on our mules. When we dismounted, we were at a height of 4945 metres. The surface was now quite impassable for mules. Besides, these animals endeavoured to make us sensible of their fatigue with a truly remarkable instinct; the ears which are generally erect, were now turned downwards, and during the numerous pauses they made for the purpose of breathing, they did not cease looking on the plain beneath. Probably few riders have reached so great a height, but several years' instructions in riding on the Andes are necessary, in order to enable one to arrive at the insecure surface beyond the snow limits.

After an examination of the locality in which we were, we saw that in order to ascend to the ridge which leads to the summit of Chimborazo, it would be necessary to clamber up an unusually steep acclivity before us. It consisted chiefly of blocks of rock of all dimensions heaped on one another. Here and there these fragments of trachyte were covered with more or less extensive surfaces of ice, and at several points it could be perceived that the rocky debris rested on hardened snow. These masses, therefore, owed their origin to recent fallings down of the rock which had taken place in the upper part of the mountain. These descents of rocky masses occur frequently, and take place in the midst of the glaciers of the Cordilleras, so that one might almost doubt whether the avalanches consisted more of stone or of snow.

It was a quarter from eleven when we dismounted from our mules. So long as we walked over rocks, we experienced no great difficulty. We ascended as if it were on a bad ladder; and the most troublesome part of our labour was the continued care with which it was necessary to select the stones upon which we could with safety place our feet. Every six or eight paces we paused to breathe, without, however, sitting down. Sometimes I employed the time afforded by the delay in collecting rocks for my geognostical collection. But whenever we reached the snow, the heat of the sun became oppressive, breathing difficult, and consequently resting more frequently necessary.

At a quarter before twelve we traversed obliquely an extensive field of ice, in which, for the sake of security, it was necessary to make steps. This portion of our journey was not without danger; a slide would have cost us our lives. We now once more stepped on fragments of trachyte—hard ground for us—and on which we ascended somewhat more rapidly. We walked one behind the other, first myself, then Colonel Hall, and last my Negro, who followed exactly in our footsteps, so that the instruments entrusted to his care might run no risk. We preserved perfect silence during our march, for experience had taught me that at such a height nothing is more hurtful than a continued conversation; and when we exchanged a few words during a halt, it was in a low tone of voice. It is chiefly to this foresight that I attribute the good health which I have invariably enjoyed during all my ascents to volcanos. I impressed, in a despotic manner, this salutary precaution on my companions. An Indian who neglected this advice on Antisana, by calling with all his force to Colonel Hall, who had lost the proper path while passing through a cloud, was in consequence attacked by giddiness and hæmorrhage.

We soon attained the ridge along which we were to ascend. It was not the same we had seen from a distance. There was indeed little snow on it, but it presented acclivities which were ascended with great difficulty. Great exertions were necessary, and leaping is difficult in these regions.

We at length reached the foot of a steep wall of trachyte, several hundred metres in height. It was a moment of discouragement when the barometer informed us that we had only attained the height of 5680 metres. This was little for us, for it was not even the height to which we had arrived on Cotopaxi. Besides, Humboldt had ascended to a greater elevation, and we wished at least to reach the station where that learned traveller stopped. Those who ascend mountains are always much inclined to sit down after meeting with discouragement, and we placed ourselves on the station of the *Peña colorado* (the red rock). It was the first repose we had enjoyed. We all suffered from excessive thirst, and in order to satisfy it, our first occupation was to suck fragments of ice.

It was a quarter from 1, and we experienced considerable cold. The thermometer had fallen to 0°4 cent. We were enveloped in a cloud. The hair-hygrometer indicated 91°5; and after the cloud was dispersed it remained at 84°. Such a degree of moisture at so great a height might appear remarkable; but I have often remarked the same thing on the Andes, and it seems to be quite capable of explanation.

During the day the surface of the snow is generally moist; the rock of Pēña Colorada, for example, was quite wet; the air immediately round the glacier might therefore be saturated with moisture. On Mont Blanc Saussure saw his hygrometer stand at between 50° and 51°, while the temperature varied from 0°5 to 2°3 R. It is not rare, even at the surface of the sea, to meet with a similar hygrometrical condition of the air. In the Cordilleras there are great drynesses on the plateaus of 2000 or 3500 metres in height. At Quito and Santa Fé de Bagota, as I have mentioned in another memoir *, the hygrometer has been observed to fall to 26°.

The misfortunes which have befallen people who visited glaciers, and especially the frequent deep cracks of the skin of the face, cannot, therefore, in my opinion, be produced by the extreme dryness of the air. These injuries seem to me, at least chiefly, caused by the too strong light, since, in order to preserve the face from all rents, it is merely necessary to cover it with a simple piece of coloured crape,—a substance so open in its texture, cannot, it is evident, protect the skin from the air, but it is sufficient to moderate the intensity of the light to which one is exposed when the sun shines on the surface of the snow. I have been assured that it is sufficient to blacken the face in order to prevent the disagreeable effect of the light. I am so much the more inclined to believe this, as the Negro who accompanied me to Antisana, suffered, it is true, like myself, a violent inflammation of the eyes, from neglecting to wear a veil, but his face was not at all affected, while mine was quite disfigured.

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* *Récherches sur la Cause qui produit le Goître. &c. Annal. de Chim. et de Phys.* v. xlviii. p. 41.

When the cloud by which we were surrounded disappeared, we examined our halting-place. Looking towards the red rock we had on our right hand a frightful precipice, and on our left, towards Arenal, we observed a projecting rock, which resembled a "*Belvedere.*" It was important to go there in order to ascertain if it were possible to go round the red rock, and, at the same time, if we could ascend higher. The approach to this "*Belvedere*" was dangerous, but I reached it with my two companions. I now perceived that if we were able to climb up a very steep surface of snow, which was on one side of the red rock, in the opposite direction from the point at which we first arrived, we should be able to attain a much more considerable elevation. In order to form a somewhat correct idea of the topography of Chimborazo, let the reader imagine an enormous mass of rock, supported on all sides by buttresses. These buttresses are the ridges which seem to extend from the plain to the immense block in order to support it.

Before commencing this dangerous expedition I ordered the Negro to examine the snow. It was of suitable consistence. Hall and the Negro advanced, I followed, when they stood firmly enough to be able to receive me, for, in order to join them again, it was necessary to slide down 25 feet on the ice. At the moment when we again commenced our march, a stone descended from the mountain above, and fell close to Colonel Hall. He stumbled and fell. I thought he was wounded, and was not satisfied till I saw him rise, and had looked with the glass at a fragment of the stone, which had arrived so opportunely for examination. This unlucky fragment of trachyte was identical with that on which we had been walking.

We advanced cautiously. To the right we could halt at the rock; to the left the abyss was frightful. Before proceeding further, we endeavoured to make ourselves acquainted with the precipice. This is a precautionary measure amongst mountains, which should never be neglected when we arrive at a dangerous position. Saussure said this long ago, but it cannot be too often repeated. During my excursions in the Andes, I have always kept this rule in view.

We now began, more than had previously been the case, to experience the effects of the rarefaction of the air. We were compelled to stand still every two or three steps, and often even to sit down for some seconds. But so soon as we had sat down we again rose up; for our suffering only lasted while we were in motion. Soon the snow assumed a character which rendered our progress, however slow, dangerous. The snow was soft, and was hardly three or four inches thick; under it there was a very hard and very smooth ice. We were obliged to cut steps, in order to have a secure footing. The negro went before to perform this work, but it exhausted him. As I was endeavouring to pass him, in order to relieve him, I slipped, but, fortunately, was vigorously supported by Hall and my negro. For an instant we three all stood in the greatest danger. This accident rendered us for a short time undecided; but we soon acquired fresh courage, and resolved to advance. The snow became more favourable, we exerted all our energies, and at a quarter before four, we arrived at the longed for ridge. We found ourselves at the foot of a trachyte prism, whose upper surface, covered by a dome of snow, formed the summit of Chimborazo.

The ridge upon which we had ascended was only a few feet in breadth. On all sides we were surrounded by precipices; round about us the most singular neighbourhood presented itself. The dark colour of the rock was contrasted in the most remarkable manner, with the dazzling whiteness of the snow. Long icicles seemed to be suspended over our heads. One might have said that a splendid waterfall had been frozen. The weather was delightful; only in the west a few small clouds appeared. The air was perfectly still; the view boundless. Our situation was new, and afforded us the most lively satisfaction.

We were at an absolute height of 6004 metres, the greatest elevation to which, I believe, man has hitherto attained on mountains.

At two o'clock the barometer stood at 371.1 lines (13 inches 8.5 Lin. Fr.) the mercury being at 7°.8 cent. In the shade of a rock, the unattached thermometer stood also at 7°.8 cent. I sought, but in vain, for a corner, in which I might be able to ascertain the mean temperature of the station. One foot under the snow the thermometer stood at 0° cent.; but this snow was in a melting state, so that it could not afford any other result.

After a few instants' repose we had entirely recovered from our fatigues. None of us experienced the sufferings of which most persons complain during the ascent of mountains. Three quarters of an hour after our arrival, Colonel Hall's pulse beat 106 times in a minute. We were thirsty; we were evidently in a state of slight fever; but this was not at all troublesome. My friend was extravagantly lively, and his humour inexhaustible, while he was occupied in sketching our ice-hell, as he termed our neighbourhood.

The voice of my companion was so much altered, that, under all other circumstances, it would have been impossible to recognise it. The feeble sound which the strokes of my hammer made, even when I struck the rocks with redoubled strength, astonished us exceedingly.

The rarity of the air generally produces remarkable effects on persons who ascend high mountains. Saussure was indisposed at the summit of Mont Blanc, and had an inclination to faintishness. His guides, who were all natives of the Valley of Chamouny, were affected in the same manner. This indisposition increased when he moved, or when, while observing his instruments, he directed his attention to a particular object. The first Spaniards who attempted the ascent of the high mountains of America, were, as D'Acosta reports, attacked by sickness and pain in the bowels. Bonguer had several hemorrhages, on the Cordilleras of Quito, and Zumstein suffered from the same cause on Monte Rosa. Humboldt and Bonpland, during their ascent of Chimborazo, on the 23d June 1802, felt a tendency to vomit, and the blood rushed from the lips and gums. In regard to ourselves, we experienced, it is true, so long as we ascended, a difficulty of breathing and unusual weakness; but we got free of this evil when we ceased moving. And when we sat down, we believed that we were in our usual state of health. Perhaps our insensibility to the action of rarefied air is to be ascribed to our long residence in the high lying towns of the Andes. When we have seen the activity exhibited in towns like Bogota, Micuipampa, Potosi, &c. which stand at a height of 2600 to 4000 metres; when we have witnessed the strength and wonderful activity of the Torcadores, in bull-fights at Quito, which is at an elevation of 3000 metres; when we have seen young and delicate females dance the whole night in places nearly as high as Mont Blanc, where the celebrated Saussure hardly retained strength enough to observe his instruments, and where his robust mountaineers fainted when they attempted to dig a hole in the snow; and, finally, when we remember that a celebrated battle, that of Pichincha, was fought nearly at the height of Monte Rosa,—it will, I believe, be agreed that man can become accustomed to breathe the rarefied air of the highest mountains.

During all the excursions which I made in the Cordilleras, the ascent to the same height over snow has been more oppressive than when it has been on naked rock. We suffered much more when climbing up Cotopaxi, than during our ascent of Chimborazo. On Cotopaxi we were constantly on snow. The Indians also of Antisana assured us that they experienced an oppression (*ahogo*), when they marched for a long time on surfaces of snow; and I confess that I am much inclined to attribute, at least partly, to this still unknown action of the snow, the disagreeable feelings experienced by Saussure and his companions, during their bivouac on Mont Blanc, at the moderate height of 3888 metres. Bivouacs, even at the height of the towns of Calamarca and Potosi, are not attended by any disagreeable consequences *. On the mountains of Peru, in the Andes of Quito, travellers, and also the mules on which they ride, experience occasionally, and very suddenly, great difficulty in breathing; it has been asserted that mules have been seen to fall down in a state of asphyxia. This does not always take place; and, in many instances, the occurrence of these symptoms seems unconnected with the effects of rarefied air. They have been remarked chiefly when there has been much snow on the mountains, and when the weather was tranquil. This perhaps is the proper place to remark, that Saussure felt himself relieved from his uncomfortable sensations, when a gentle north-east wind began to blow. In America, the meteorological condition which attacks the respiratory organs so severely is termed *Soroche.* In the language of the American miner, *Soroche* means iron-pyrites, an indication that the cause of this phenomenon has been sought in subterranean exhalations. This is not an impossible cause, but it is more natural to consider the soroche as an effect produced by the snow.

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* According to Mr Pentland, Calamarca is placed at a height of 4141 metres, and the highest part of Potosi at 4166 metres.

The feelings of difficulty of breathing which I myself have several times suffered on snow when the sun was shining on it, have suggested the conjecture that an impure air is evolved by the action of the heat of the sun on the snow. This singular idea was confirmed by an old experiment mentioned by Saussure, according to which he believed he had discovered that the air evolved from the pores in the snow contained less oxygen than the atmosphere. The air used for the examination was taken from the pores of the snow collected on the Col du Géant. The analysis was made by Sennebier, by means of nitrous oxide gas, and compared with that of the air of Geneva. The results as reported to us by Saussure were the following:—

"In Geneva a mixture of equal portions of atmospheric air and nitrous oxide gas yielded, on two trials, 1.00 residue. The air from the snow yielded one time 1.85, and a second 1.86 residue. These trials, which seem to indicate great impurity of the air, would require further experiments to ascertain the nature of the gas which in this air occupied the place of the oxygen."

For a long time I had cherished the desire to repeat the experiment of Sennebier; for if it were actually true that the air of mountain snow contains less oxygen than common air does, we should be able to understand how the impure air evolved by the action of the heat of the sun, by being dispersed through the atmosphere, could oppress those persons who were obliged to breathe it. With this view I filled a flask with snow at the station of *Chillapullu.* When we returned to the farm of Chimborazo, the snow was entirely melted, and the water so produced occupied about an eighth of the flask; a space of seven-eighths,

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[*] Saussure, *Voyage dans les Alps,* vol. vii. p. 472.

therefore, was filled with air which was chiefly derived from the snow. I say chiefly, because a considerable portion of atmospheric air must have entered during the filling of the flask with snow.

I decomposed the air from the snow of Chillapullu very carefully, by means of a phosphorus eudiometer.

Eighty-two parts of the snow air left a residue of sixty-eight parts nitrogen. Fourteen parts oxygen were therefore absorbed, and consequently the air contained 0.17 oxygen.

When we consider that the flask must, besides the air of the snow, have also contained atmospheric air, we are inclined to regard this analysis as a confirmation of the results obtained by Saussure; and the difficulty of the breathing on the glaciers on which the sun is shining, the soroche of the high mountains of Peru, would be to a certain extent explained, if we admit that the air surrounding a glacier is in the immediate vicinity of that glacier less pure than that of the atmosphere.

The eudiometrical result which I obtained is doubtless free from objections, but nevertheless further experiments are necessary to prove distinctly that the air which I analyzed was exactly the same as that obtained from the pores of the melted snow. In fact, in order to procure this air, it was necessary for me to wait for the melting of the snow. The air in the flask was therefore in contact with that water containing more or less air, which resulted from the melting. But it is known that, under such circumstances, the water absorbs more oxygen than nitrogen, and that consequently the air with which the water is saturated always contains more oxygen than atmospheric air does. The air which remained in the flask, and which was that examined by me, might therefore contain less oxygen, notwithstanding that, in fact, the air contained in the snow might have the usual composition. This is the objection which, in strictness, may be made to my result. In order to judge of Saussure's result, it would, above all, be necessary to know what method that celebrated traveller pursued, in order to obtain from the snow the air examined by Sennebier.

The philosophers who have visited high mountains, agree in stating that the blue colour of the sky appears darker the greater the height attained. On Mont Blanc, Saussure saw the sky having the deepest king's blue tint;* and at night, during a bivouac on the same mountain, the moon, as he himself expressed it, shone with great brilliancy in a heaven as black as ebony.

On the Col du Géant the intensity of the colour was even more striking. Saussure invented an instrument in order to make comparisons of such observations. At our station at Chimborazo, the heaven, which, on our arrival was of remarkable purity, seemed to us to present no darker tint than that of the sky at Quito. But as I had an opportunity of seeing the sky of almost a black colour at a much lower elevation, I now report the facts as I have observed them.

When I was on Tolima, the sky had its usual colour, although I was at a height of 4686 metres, therefore little under the snow line.

On the Volcano of Cumbal the sky seemed to me of a remarkably deep indigo-blue colour. I was then surrounded by snow, for the dome of the volcano is crowned with a glacier. During the whole of my ascent to the Cumbal, so long as I had not reached the snow boundary, the colour seemed much less dark. This blackness alarmed the Negro who carried my barometer. In the evening we both were attacked by inflammation of the eyes, which rendered us blind for several days.

When I ascended Cotopaxi, I provided myself and my companion with coloured glasses. After we had wandered for five hours on snow, we halted at an elevation of 5716 metres. The heaven, as seen by the naked eye, did not seem to us darker than it did from the plain, just as we found on Chimborazo, the sky of Rio-Bamba and Quito. I will not, however, deny, that, in reality, the sky, as seen from high mountains, is darker than when seen from the surface of the sea; I did not possess a cyanometer, and am, besides, quite inclined to admit the correctness of the general results attained by Saussure with this instrument. I merely assert that this difference of colours is only to be remarked by comparison, and that the blackness of the sky which has been sometimes observed on glaciers, has been occasioned by a dulness in the organs of vision, and also, perhaps, by the operation of an easily understood contrast.

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* Saussure's Voyage, vol. vii. p. 321.

The mountaineers who accompanied Saussure in his memorable ascent to Mont Blanc, asserted, that they had seen stars in clear day; this it was, which, on the ascent, guided them to the top of the mountain. Saussure himself did not witness this phenomenon, his attention was at that time directed to other objects; but he intimated no doubt of the truth of the unanimous testimony of his guides. Neither on Chimborazo, nor, I can add, on any other mountain of the Andes, on which range I have attained elevations much greater than those to which Saussure ever reached on the Alps, have I seen stars during the day; and yet I was often in the most favourable situations for the purpose; as for example, on the Péna Colorado, where I was in the shade, at the foot of a very thick wall of trachyte.

While we were occupied with our observations on Chimborazo, we had uninterrupted fine weather, and the sun was so hot as even to annoy us a little. About three o'clock we saw some clouds forming in the plain; soon the thunder rolled under our feet, and we believed at first it was a *bramido*, or subterranean sound. Not long after, encircling the foot of the mountain, the clouds began to ascend towards us; we had no time to lose, for it was necessary to pass the dangerous part of our journey before they should overtake us, or else be exposed to great danger. A greater fall of snow or a frost, which would have made our route slippery, would have been sufficient to delay our return, and we had no provisions to enable us to spend the night on the glaciers.

The descent was difficult. After descending 300 to 400 metres, we encountered a cloud. A little lower down it began to hail, and the air was thus considerably cooled. At the moment we again found the Indians, who took charge of our mules, the cloud poured down upon us a hail of such size, that we experienced from it pain both on our hands and faces.

At a quarter from five I opened my barometer at the Pedro del Almuerzo; there, in the morning, at nine o'clock, it had stood at $457^{\text{m.m.}}$ 6; the mercury indicating 10° cent.; the air 5° 6 cent. I found it at $4\frac{3}{4}$ o'clock at $458^{\text{m.m.}}$, 2; the mercury being 4° 8 cent., and the air 3° 9 cent.; the difference, therefore, $000^{\text{m.m.}}$, 6.

It is singular enough, that, at this height, the daily variation should be the reverse of what it generally is under the tropics; that is, that the barometer, from nine o'clock in the morning to four o'clock in the afternoon, had risen instead of falling. This irregularity arose probably from an accidental circumstance; I am so much the more inclined to believe this, as I found that there was the usual variation at the farm of Antisana, though it was less considerable than in the plain.

As we descended, an icy rain became mixed with the hail. The night surprised us on our route; it was eight o'clock when we arrived at the farm of Chimborazo.

The geological observations which I was able to make during this expedition, all tend to confirm the ideas which I have in another place expressed on the nature of the trachyte, which forms the ridge of the Andes; for on Chimborazo all the facts were again presented to me which I have adduced in my description of the Equatorial volcanos. This is evidently an extinct volcano like Cotopaxi, Antisana, Tunguragua, and, in general, all the mountains standing on the plateaus of the Andes. Chimborazo consists of an assemblage of irregularly piled up masses of trachyte. There, frequently, enormous blocks of trachyte have been sent up from the volcano in a hard condition; their edges are sharp; nothing indicates that they have once been in a melted or soft state. There is nowhere to be seen on any of the volcanos an approach to a stream of lava. Nothing is ejected from these craters but masses of mud, elastic fluids, and red-hot fragments of trachyte, which have more or less the character of scoriæ, and are often thrown to very considerable distances.

A plateau forms the foot of Chimborazo, which may be studied in detail in the rivulets near the farm. There I ascertained that the trachyte is not at all arranged in strata, but is traversed by fissures in all directions. This rock consists chiefly of felspar, which is generally of a grey colour, and includes crystals of augite and a glassy felspar.

The trachyte rises towards Chimborazo, and often presents considerable rents, which become deeper and broader as they approach the mountain. One might say that Chimborazo, as it rose, had burst the plateau which serves as its basis.

The trachytic rock, which constitutes the greater part of the province of Quito, presents few variations. The confused heaped blocks, which form the volcanic cones, are of the same mineralogical nature as the rock which is their foundation. These cones and steep mountains were doubtless elevated by elastic fluids, at the points where there was the least resistance. The trachytic rock, which is shattered into innumerable fragments, has, as it was brought to the surface, been elevated by the vapours which were evolved. After the eruption, the shattered rock must have acquired a larger volume, as all the masses could not return to the place whence they came; they, therefore, were heaped over the opening through which the evolution of the gas had taken place.

It is precisely what would happen if we were to dig a well in a hard and compact rock, and then wished to throw in the masses of stone thus obtained; the well would soon be filled, and if we were to continue heaping up the stones in the same direction, a cone would be formed over the mouth of the well, which would be higher the deeper the excavation. In this manner, I think, Cotopaxi, Tunguragua, Chimborazo, &c. have been formed.

The elastic fluids, which, after they had broken up the trachytic crust, opened through it a passage for themselves, might on the surface of the ground unite into more or less considerable cavities, hollows already in existence; and we can thus conceive that the masses of rock, which were at first raised, might afterwards sink and occupy these hollows. Thus a cavity must have been formed on the surface, in place of a cone elevated on the point of eruption. I can thus understand the wonderful sinkings presented by the crater of Rucuipichincha, and also the green lake of the solfatara of Tuqueres, of which I have given, in another memoir, a full description.

I consider, then, the formation of the trachytic cones of the Cordilleras as posterior to the elevation of the mass of the Andes. But these cones are not the newest elevations which occur in these mountains. In the neighbourhood of the highest peaks, namely, those of Cayambé, Antisana, and Chimborazo, small mountains are to be observed, consisting of masses of rock of a newer description, and differing distinctly from the usual trachyte. It is black, porphyritic, and its basis, which includes crystals of glassy felspar, is coloured by augite; the crystals of felspar are rather rare, and the mass has often the air of a basalt; but I have never seen olivine in it. Sometimes this rock is compact and arranged in prisms; sometimes filled with holes like scoria. One would then be inclined to regard it as lava, if it covered considerable spaces; but it occurs always in fragments, which rarely have the size of a fist. This rock is evidently of very recent origin. At Chorrera de Pisque, near Ibarra, there is a beautiful colonnade, reposing on alluvium; on the estate of Lysco, this substance, in the condition of fragments, has formed a passage for itself through the trachyte, which has been elevated by its agency. It is there where Humboldt believed he saw a stream of lava (*coulée*), which had issued from Antisana. In another memoir I have explained the reasons which have induced me to differ from the opinion of my illustrious friend. The extinct volcano of Calpi, placed at the foot of Chimborazo, also consists of this kind of basalt. We visited it on our return from Rio-Bamba.

In the midst of the sand which covers the whole plain of Rio-Bamba, there is a hill of a dark colour, called the Jana-urcu (the black mountain).

At the lower part of this hill trachyte projects through the sand; it is of the same nature as that on which, at some distance, Chimborazo rests. This trachyte seems to have been thoroughly shattered; it is full of clefts and fissures in all directions. The acclivity of the Jana-urcu, towards Calpi, consists of small fragments of the black rock, whose heaping together completely reminds one of the stone-eruption of Lysco. It appears, indeed, that this eruption of the Jana-urcu, took place after the deposition of the sand which covers the plain; for in the vicinity of the volcano the ground is covered with black scoriaceous stones.

Our guides, Indians from Calpi, conducted us to a cleft, where the sound of a subterranean waterfall was distinctly heard; and, judging from the loudness of the noise, the mass of water must be considerable.

The unproductiveness of the soil, from Latacunga to Rio-Bamba has often surprised me.

I asked myself why the glaciers of the high mountains, which tower above this district, do not give rise to numerous streams. However, the dryness of this plateau is merely superficial; it is certain that the water of these mountains penetrates the loose earth, and then circulates to a greater or less depth in the interior. The subterranean waterfall of Jana-urcu is a proof of this, and further proofs are afforded by the wells, often very abundant, which are to be seen when descending the deep ravines which furrow the alluvial district of this plateau.

Close to Latacunga, between that town and Cotopaxi, there is a well, which was found a few metres under the surface, during an excavation made in the pumice-conglomerates. It was called Timbo-pollo by the Indians. In reality it is not a well, but a subterranean river; but the water is constantly renewed, and even the direction of the stream can be distinctly observed. The temperature of this stream is 18°.8 cent.; the mean temperature of Latacunga is 15.°5 cent.

On the 21st December we returned to Rio-Bamba, where I remained a day, in order to finish the observations I had projected.

On the afternoon of the 23d I left Rio-Bamba, following the route to Guayaquil, where I joined the ship which conveyed me to the coast of Peru.

In sight of Chimborazo I parted from Colonel Hall, whose confidence and friendship I enjoyed during the whole of my residence in the province of Quito. His accurate knowledge of the localities was of the greatest consequence to me, and I found in him an excellent and indefatigable travelling companion. We had both for a long time served the cause of independence. Our taking leave was affecting: there was something which seemed to say we should never meet again; and unhappily this mournful presentiment was too well founded. A few months afterwards my unfortunate friend was murdered in the streets of Quito.—*Poggendorf's Annalen der Physick und Chemie*, 1835, No. 2.

On the Characters of certain groups of the Class Personatæ. By David Don, Esq., Libr. L. S., &c. Communicated by the Author.

A considerable time has elapsed since I first commenced the study of the various orders composing this important class of plants, and my attention having been again more recently directed to the subject, I have thought that the observations which I have made might not prove unacceptable to botanical readers.

The very close relationship that subsists between the several families and groups of this class renders the circumscription of them extremely difficult, and, indeed, in one like the present, exhibiting a series of natural affinities almost uninterrupted, we can scarcely hope to make out an absolute definition of any of them. Nevertheless, the division of a large class into smaller groups affords greater facilities to their study, and is advantageous in pointing out their peculiarities of structure, and the degrees of development in the various organs.

In order to render this communication more useful in a practical point of view, I shall proceed to treat of the several families separately, and conclude by giving the technical characters of each.

SCROPHULARINEÆ.

I have had occasion elsewhere * to remark of the *Rubiaceæ*, that they appeared to constitute a grand central point of union between many families of the monopetalous class; an observation which may be applied with equal justice to the family now under consideration, from the great diversity of form found in it, and from its partaking, both in habit and structure, of those orders to whom it is more immediately allied: for example, the group of *Scrophularieæ* closely approximates them to *Verbascineæ* and *Solaneæ*; the *Gratioleæ* connects them with *Lentibulariæ*; the *Antirrhineæ* and *Gerardieæ* with *Cheloneæ* and *Bignoniaceæ*; the *Calceolarieæ* with *Gesneriaceæ*; the *Euphrasieæ* with *Rhinanthaceæ*; and their affinity to *Verbenaceæ* is clearly established through the *Ruddlejeæ*. The *Verbascineæ* approach so closely to the first group as to be only distinguished by their symmetrical flowers. The normal *Solaneæ* are distinguished by the plaited æstivation of their corolla, mostly regular flowers, and by their curved embryo; and the *Cestrineæ* by their valvate æstivation and regular flowers. As neither of the groups above mentioned conveys an adequate idea of the order, I could have wished, therefore, that the name of *Personatæ* had been retained for it instead of that of *Scrophularineæ*, especially as *Scrophularia* is to be regarded as an aberrant genus.

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* Linn. Trans. 17. p. 109.

RHINANTHACEÆ.

This family, consisting of *Rhinanthus*, *Pedicularis*, *Melampyrum*, and *Cymbaria*, is intimately allied to the preceding order, but limited as here proposed, it is satisfactorily distinguished by its inverted embryo, with the radicle directed to the opposite extremity of the seed. The *Euphrasieæ* have been included by most botanists in this family, and were they to be retained, the separation between it and the preceding order would be rendered impossible. *Cymbaria* differs from the rest of the order in the very considerable degree of development of its embryo.

OROBANCHEÆ.

A very natural family, well distinguished from the preceding by their persistent corolla, unilocular ovarium, centrifugal embryo, and peculiar economy, in being for the most part parasitical herbs, destitute of proper leaves, and of the green colour common to other plants. The seeds are attached to the narrow depressed placentæ, formed by the united edges of the valves. The embryo is placed in the ventral side of the seed, and at the opposite extremity from the hilum, resembling a minute nearly globular homogeneous mass, and, as far as I have been able to observe, perfectly undivided, and consequently devoid of true cotyledons. In all these respects *Obolaria* and *Tozzia* seem to agree, but differ in having proper leaves, and in being terrestrial.

CHELONEÆ.

This group is well entitled, in my opinion, to be retained apart from the *Scrophularineæ*, on account of the greater degree of development of its embryo, its compressed winged seeds, and of the presence of a rudimentary stamen, all of which characters bring it near to the *Pedalineæ* and *Bignoniaceæ*, from which it is distinguished by its albuminous seeds.

ARAGOACEÆ.

The single genus constituting this group, is one of the most remarkable in point of structure in the whole class. Its regular symmetrical flowers, the twisted æstivation of its corolla, and the structure of its seeds, bring it very near to the *Polemonia-*

ceæ, especially to the genus *Diapensia*, with which it has many points in common, both in habit and structure, and its true place in the system of nature is evidently between that family and *Scrophularineæ*.

SIBTHORPIACEÆ.

In my observations on the genus *Codon*, given in a former number of this Journal, I have already proposed to separate *Sibthorpia* and *Disandra* as a distinct group, intermediate between *Scrophularineæ* and *Primulaceæ*. To the above genera I would now add *Romanzovia*, *Capraria*, and *Scoparia*. They are chiefly distinguished from *Scrophularineæ* by their regular symmetrical flowers.

SCROPHULARINEÆ. *Brown.*

PERSONATARUM GENERA, *L.*

SCROPHULARIARUM ET PEDICULARUM PARS, *Juss.*

CALYX 4-5 divisus, persistens.
COROLLA monopetala, hypogyna, decidua, irregularis, æstivatione imbricatâ.
STAMINA plerumque 4, didynama, quandoque 2, nunc rudimento quinti.
PISTILLUM 1. *Ovarium* biloculare. *Stigma* bilobum v. indivisum.
CAPSULA (rarò Bacca) bilocularis, 2-4 valvis, polysperma.
SEMINA parva, albumine copioso prædita.
EMBRYO teres, erectus, inclusus, rectus. *Radicula* ad umbilicum spectans, infera.
Herbæ v. Frutices polymorpha, sæpiùs oppositifolia. Inflorescentia varia.

Trib. 1. SCROPHULARIEÆ. *Calyx* profundè 5-partitus: *segmentis* plerumque dilatatis, margine scariosis, æstivatione imbricatis. *Corolla* tubulosa v. campanulata, ventricosa: *limbo* 5-lobo, bilabiato. *Stamina* 4, didynama. *Antherarum loculi* apice confluentes, connectivo carnoso inserti. *Stigma* capitatum v. leviter bilobum. *Capsula* crustacea. *Dissepimentum* duplicatum e valvularum marginibus introflexis constitutum, utrinque placentiferum. *Semina* testâ crassâ, spongiosâ, corrugatâ. *Albumen* carnosum. *Embryo* ferè albuminis longitudine.

Herbæ v. Frutices (plerumque Europæa) *foliis oppositis alternisve, floribus terminalibus racemosis flavis v. purpureis.*

OBS. Ad Solaneas, sed imprimis ad Verbascineas, tam habitu quàm charactere prope accederunt.

1. Digitalis, *L.* 2. Scrophularia, *L.* 3. Alonsoa, *Ruiz et Pavon.*

Trib. 2. ANTIRRHINEÆ. *Calyx* profundè 5-partitus, æstivatione imbricatâ. *Corolla* personata, ventricosa: *limbo* 5-lobo, bilabiato. *Stamina* 4, didynama, inclusa, quandoque quinti rudimento. *Antherarum loculi* apice distincti. *Stigma* leviter bilobum. *Capsula* crustacea. *Dissepimentum* indivisum. *Placentæ* spongiosæ, adnatæ. *Semina* testâ crassâ, spongiosâ, corrugatâ v. cellulosâ. *Albumen* carnosum. *Embryo* ferè seminis longitudine.

Herbæ v. Suffrutices (Orbis temperatis propria) *foliis oppositis alternisve, floribus terminalibus spicato-racemosis v. axillaribus solitariis.*

OBS. Semina Nemesiæ omninò ut in Lophospermo.

1. Antirrhinum, *Tourn. Juss.* 2. Linaria, *Tourn. Juss.* 3. Anarrhinum, *Desf.* 4. Maurandia, *Cav.* 5. Lophospermum, *Don.* 6. Nmesia, *Vent.* 7. Galvezia, *Domb. Juss.*

Trib. 3. GRATIOLEÆ. *Calyx* sæpiùs tubulosus, 5-fidus. *Corolla* tubulosa, ventricosa, quandoque personata: *limbo* 5-lobo, bilabiato. *Stamina* 4, didynama, inclusa, nunc 2 tantùm fertilia. *Antherarum loculi* divergentes, apice distincti. *Stigma* plerumque bilamellatum. *Capsula* sæpè membranacea, rarò subunilocularis. *Dissepimentum* membranaceum, e valvularum marginibus introflexis constitutum. *Placentæ* spongiosæ, demùm liberæ. *Semina* testâ subcrustaceâ. *Albumen* carnosum. *Embryo* ferè seminis longitudine.

Herbæ v. rarò Suffrutices (Orbis temperatis v. locis paludosis intra Tropicos incola) *foliis oppositis verticillatisve, floribus axillaribus terminalibusve sæpius luteis aut purpureis.*

1. Gratiola, *L.* 2. Herpestis, *Gærtn.* 3. Lindernia, *L.* 4. Conobia, *Aubl.* 5. Limnophila, *Br.* 6. Hydrotriche, *Zucc.* 7. Cybbanthera, *Ham.* 8. Mazus, *Lour.* 9. Morgania, *Br.* 10. Mimulus, *L.* 11. Leucocarpus, *Don.* 12. Torenia, *L.* 13. Artanema, *Don.* 14. Collinsia, *Nutt.* 15. Hemianthus, *Nutt.* 16. Limosella, *L.* 17. Angelonia, *Bonpl.* 18. Stemodia, *L.* 19. Dodartia, *L.*

Obs. Collinsiæ et præsertim Hemiantho affinis est Limosella, et subinde tribus ad Lentibularias accedit.

Trib. 4. GERARDIEÆ. *Calyx* tubulosus, 5-dentatus. *Corolla* campanulata, ventricosa: *limbo* 5-lobo, bilabiato. *Stamina* 4, didynama, sæpè exserta. *Antheræ* biloculares, sæpè barbatæ: *loculis* divergentibus, apice distinctis, basi plerumque calcaratis. *Stigma* clavatum, indivisum. *Capsula* bipartibilis. *Dissepimentum* e laminis 2 connatis compositum. *Placentæ* angustæ, spongiosæ. *Semina* angulata, testâ crassâ favoso-cellulosâ. *Albumen* carnosum. *Embryo* subfoliaceus, seminis ferè longitudine. *Radicula* brevissima, obtusa.

Herbæ *v.* Suffrutices (plerumqe americana), *foliis oppositis scabris, floribus axillaribus solitariis v. terminalibus spicato-racemosis flavis aut purpureis.*

1. Gerardia, *L.* 2. Conradia, *Nutt.* 3. Seymeria, *Pursh.* 4. Sopubia, *Ham.* 5. Centranthera, *Br.* 6. Gastromeria, *Don.* 7. Virgularia, *Ruiz et Pavon.*

Trib. 5. VERONICEÆ. *Calyx* 4–5-partitus, æstivatione imbricatâ. *Corolla* rotata 4-loba, v. tubulosa subbilabiata. *Stamina* 2 v. 4, exserta, distantia. *Antheræ* incumbentes: *loculis* parallelis, distinctis. *Stigma* capitatum. *Capsula* membranacea. *Dissepimentum* angustum, e laminis 2 separabilibus compositum. *Placentæ* brevissimæ, septi medio insertæ. *Semina* compressa, testâ membranaceâ. *Albumen* cartilagineum. *Embryo* minutus, teres, in basi albuminis locatus.

Herbæ *v.* Suffrutices (utriusque hemisphærii zonis temperatis communia), *foliis plerumque oppositis, floribus axillaribus terminalibusve solitariis s. racemosis cæruleis aut albis.*

1. Veronica, *L.* 2. Leptandra, *Nutt.* 3. Pæderota, *L.* 4. Wulfenia, *Jacq.* 5. Picrorhiza, *Royle.* 6. Gymnandra, *Pall.* 7 ? Antonia, *Br.*

Trib. 6. BUCHNEREÆ. *Calyx* tubulosus, striatus, 5-fidus. *Corolla* hypocrateriformis, limbo æquali. *Stamina* 4, didynama, inclusa. *Antheræ* e loculis continuis plerumque unilobæ, connectivo crasso, elevato. *Stigma* indivisum. *Capsula* septo simplici. *Placenta* angusta, columnaris, longitudinaliter adnata, v. demùm libera. *Semina* minuta, lævia v. scrobiculata, testâ membranaceâ. *Albumen* carnosum. *Embryo* semine multò brevior.

Herbæ *v.* Suffrutices (plerumque capensia), *foliis oppositis alternisve, floribus terminalibus spicato-racemosis albis v. rubris.*

1. Buchnera, *L.* 2. Manulea, *L.* 3. Nycterinia, *Don.* 4. Erinus, *L.* 5. Escobedia, *Ruiz et Pavon.*

OBS. Manulea tomentosa transitum ad tribum sequentem planè reddit.

Trib. 7. BUDDLEJEÆ. *Calyx* campanulatus, 5-dentatus. *Corolla* tubulosa: *limbo* 4–5-fido, æquali, patulo. *Stamina* 4–5, subæqualia, inclusa.

Antheræ e loculis 2 parallelis distinctis compositæ. *Stigma* clavatum, bilobum. *Capsula* crustacea. *Dissepimentum* e marginibus valvularum introflexis constitutum, placentæ crassæ spongiosæ insertum. *Semina* angulata, scobiformia, testâ laxâ membranaceâ. *Albumen* carnosum. *Embryo* ferè seminis longitudine: *cotyledones* oblongæ, compressæ: *radiculâ* brevissimâ.

Frutices *ramis quadrangulis, foliis oppositis reticulato-venosis, floribus terminalibus capitatis spicatis aut paniculatis plerumque aurantiacis.*

OBS. Tribus Verbenaceis plurimis analoga habitu sat simili confirmata.

1. Buddleia, *L.*

Trib. 8. CALCEOLARIEÆ. *Calyx* 4-partitus, æstivatione valvatâ. *Corolla* irregularis, bilabiata. *Stamina* 2 v. 4, fauci inserta. *Antherarum loculis* basi divergentibus, apice confluentibus. *Stigma* indivisum, capitatum. *Capsula* membranacea. *Dissepimentum* e valvularum marginibus introflexis constitutum. *Placentæ* 2, stipitatæ v. septo adnatæ. *Semina* minuta, umbilico mucroniformi: *testâ* membranaceâ. *Albumen* carnosum. *Embryo* teres, albuminis dimidii longitudine.

Herbæ v. Suffrutices (Amer. Austr. v. Capens.) *sæpiùs glanduloso-pubescentes, foliis oppositis sæpiùs indivisis, floribus terminalibus plerumque corymboso-paniculatis v. racemosis flavis.*

OBS. Tribus naturalis omninò osculans videtur inter Scrophularineas et Gesneriaceas; his, calycis æstivatione valvatâ, capsulæ valvulis revolutis placentiferis, seminibus minutissimis funiculo umbilicali longiori præditis, habitu inflorescentiâ foliisque oppositis plerumque rugosis crenatis; illis, antheris bilobis apice confluentibus, seminibus albuminosis convenit. Calceolaria petioalaris aliæque in hortis proferunt quandoque corollas tubulosas subregulares. Ourisia habitu simillima, fortè tamen ad Gesneriaceas meliùs depellenda.

1. Calceolaria, *L.* 2. Jovellana, *Ruiz et Pavon.* 3. Bæa, *Commers.* 4. Hemimeris, *L.*

Trib. 9. EUPHRASIEÆ. *Calyx* tubulosus, 5-dentatus. *Corolla* bilabiata. *Antherarum loculis* parallelis, sæpiùs basi calcaratis. *Stigma* indivisum. *Capsula* crustacea, septo utrinque placentifero. *Semina* testâ membranaceâ. *Albumen* carnosum. *Embryo* teres, axilis, rectus, albuminis ferè longitudine.

Herbæ *foliis oppositis, floribus terminalibus subspicatis bracteatis.*

1. Euphrasia, *L.* 2. Bartsia, *L.* 3. Lamourouxia, *Kunth.* 4. Castilleja, *Mut.*

RHINANTHACEÆ. *Decand.*

MELAMPYRACEÆ. *Rich.*

PEDICULARUM PARS. *Juss.*

CALYX tubulosus, 4-5-fidus, persistens.

COROLLA tubulosa, hypogyna, decidua, irregularis, æstivatione imbricatâ; *labio superiore* plerumque galeato.

STAMINA 4, didynama, fertilia. *Antheræ* basi calcaratæ.

PISTILLUM 1. *Ovarium* biloculare. *Stigma* indivisum.

CAPSULA bilocularis, 2-4-valvis, polysperma.

SEMINA testâ membranaceâ. *Albumen* copiosum, densè carnosum.

EMBRYO inversus, teres. *Radicula* ab umbilico remota, centrifuga.

Plantæ (Europ. Asiæ et Amer. bor.) humillimæ, herbaceæ, v. raro suffruticosæ. Folia sæpiùs opposita, pinnatifida. Flores terminales, spicati, bracteati.

* *Embryo minutus in apice albuminis.*

1. Rhinanthus, *L.* 2. Pedicularis, *L.* 3. Melampyrum, *L.*

** *Embryo albuminis feré longitudine subfoliaceus.* CYMBARIEÆ.

4. Cymbaria, *L.*

OROBANCHEÆ. *Rich. Juss.*

CALYX divisus, persistens.

COROLLA tubulosa, hypogyna, irregularis, persistens! æstivatione imbricatâ.

STAMINA 4, didynama, fertilia. *Antheræ* basi sæpiùs calcaratæ.

PISTILLUM 1. *Ovarium* 1-loculare. *Stigma* bilobum v. indivisum.

CAPSULA 1-locularis, 2-valvis, polysperma.

SEMINA placentis angustis parietalibus, e valvularum marginibus conflatis inserta, minutissima: *testâ* tenuissimè membranaceâ, areolis subquadratis reticulatâ: *albumen* copiosum, cartilagineum.

EMBRYO in apice albuminis inversus, excentricus, minutus, subglobosus, plerumque indivisus! *Radicula* supera, ab umbilico remota.

Herbæ plerumque parasiticæ, aphyllæ, subcarnosæ, rufescentes, squamatæ. Flores terminales, solitarii aut spicati, bracteati.

OBS. Ordo omninò naturalis facilèque definiendus corollâ persistenti, seminum structurâ, atque habitu diversissimo. In Lathræâ et Orobanchi embryo, ut videtur, indivisus et acotyledoneus, an itaque in cæteris generibus?

* *Parasiticæ, aphyllæ.*

1. Orobanche, *L.* 2. Phelypæa, *Desf.* 3. Epiphegus, *Nutt.* 4. Lathræa, *L.* 5. Hyobanche, *L.* 6. Alectra, *Thunb.* 7. Æginetia, *Roxb.* 8. Anblatum, *Tourn.*

** *Terrestres, foliatæ.* OBOLARIEÆ.

9. Obolaria, *L.* 10. Tozzia, *L.*

CHELONEÆ.

CALYX 5-partitus, persistens.

COROLLA tubulosa, hypogyna, irregularis, decidua, limbo 5-lobo.

STAMINA 4, didynama, fertilia; *quintum* sterile. *Antheræ* biloculares, muticæ: *loculis* apice confluentibus.

STIGMA indivisum.

CAPSULA bilocularis, polysperma.

SEMINA erecta, compressa, margine membranacea. *Albumen* carnosum.

EMBRYO erectus, foliaceus.

Herbæ v. Suffrutices (Amer Septentr.). Folia opposita. Inflorescentia paniculata v. spicata.

OBS. Tribus Bignoniaceis et Pedalineis proximè affinis, calyce tamen subpolyphyllo, stigmate indiviso, seminibus erectis albuminosis facilè distinguitur.

1. Chelone, *L.* 2. Penstemon, *Mitch.*

ARAGOACEÆ.

CALYX 4-5-phyllus, persistens, basi bracteatus.

COROLLA hypocrateriformis, regularis, decidua: *limbo* 4-partito, patenti, æquali, æstivatione convolutâ.

STAMINA 4, æqualia, sinubus corollæ inserta. *Filamenta* basi barbata. *Antheræ* bilobæ: *loculis* apice omninò confluentibus.

PISTILLUM 1. *Ovarium* biloculare. *Stylus* filiformis. *Stigma* capitatum, indivisum.

CAPSULA 2-locularis, 4-valvis: *loculis* 4-spermis. *Dissepimentum* utrinque placentiferum, demùm liberum.

SEMINA erecta, imbricata, compressa, margine membranaceo-alata: *testâ* membranaceâ, reticulatâ. *Albumen* copiosum, carnosum.

EMBRYO rectus, axilis, inclusus. *Cotyledones* oblongæ, compressæ. *Radicula* teres, obtusa, cotyledonibus triplò longior, infera.

Frutices (Nov. Granat.) facie cupressinâ ramosissimi, ramis oppositis. Folia quadrifariàm imbricata, coriacea, integerrima. Flores axillares, solitarii, subsessiles, albi.

OBS. Tribus osculans Personatis, Polemoniaceis et Bignoniaceis paritèr affinis; ab his, stigmate omninò indiviso et calyce polyphyllo, ab illis, corollâ regulari, æstivatione convolutâ distinguitur. Polemoniaceis, imprimis Diapensiæ convenit corollâ regulari, æstivatione convolutâ, staminum insertione, seminibus alatis, atta mendiscrepat stigmate omninò indiviso et capsulâ biloculari.

1. Aragoa, *Kunth.*

SIBTHORPIACEÆ.

CALYX 4-8-partitus, persistens.

COROLLA rotata, 4-8-fida, regularis, decidua, æstivatione imbricatâ.

STAMINA 4-8, æqualia, corollæ lobis alterna. *Antheræ* biloculares: *loculis* parallelis.

PISTILLUM 1. *Ovarium* biloculare. *Stigma* capitatum, indivisum.

CAPSULA bilocularis, bivalvis, polysperma. *Placenta* magna, spongiosa, globosa.

SEMINA erecta, testâ membranaceâ. *Albumen* copiosum, densè carnosum.

EMBRYO teres, erectus, inclusus. *Radicula* longa, cylindracea, obtusa, umbilico obversa, centripeta.

Herbæ foliis alternis indivisis, floribus axillaribus solitariis pedunculatis.

OBS. Familia admodùm parva inter Scrophularineas et Primulaceas locum tenens. A prioribus flore regulari symmetrico et placentâ globosâ, a posterioribus staminibus corollæ lobis alternis et capsulâ biloculari facilè dignoscitur.

1. Sibthorpia, *L.* 2. Disandra, *L.* 3. ? Romanzovia, *Cham.* 4. ? Capraria, *L.* (C. biflora.) 5. ? Scoparia, *L.*

May 1. 1835.

Account of the Discovery by Purkinje and Valentin of Ciliary Motions in Reptiles and Warm-blooded Animals; with Remarks, and additional Experiments.

By WILLIAM SHARPEY, M.D., F.R.S.E., Lecturer on Anatomy. Communicated by the Author.

A REMARKABLE provision exists in many animals belonging to the inferior tribes, by which fluids are moved along the surface of different organs. The gills of the mussel afford a good example of this motion; if a portion of them be cut off and examined under water, the water will be perceived moving in a current along the surface in a constant and determinate direction; and when the piece of gill is inspected with the microscope, its surface will be found covered with minute hair-like organs or cilia, which are in a state of continual agitation or oscillatory motion, by which they impel the fluid along the surface. Drs Purkinje and Valentin of Breslaw have lately made the interesting discovery that this provision exists also in warm-blooded animals, having detected the ciliary motion on the internal surface of the oviduct in birds and the Fallopian tubes in mammalia, and of the air-passages in both. A translation of their memoir is here given, to which I beg to subjoin an account of the results which I obtained on repeating some of their observations.

I may previously mention, however, that in a paper published in 1830,* I pointed out the existence of the motion in question in the larva of the frog and salamander, in most of the tribes of mollusca, in the annelida, and in the actinia, and endeavoured to shew that it was a provision extensively prevalent among aquatic animals; serving chiefly to maintain a flow of water along the surface of their respiratory organs, but in some cases also to convey food to the animal, discharge the ova, or assist in locomotion. The distinguishing characters of this motion were, 1st, That the fluid was moved along the surface of the parts in a determinate direction; 2d, That the impelling power resided in the surface over which the fluid was conveyed, which in most instances, or, as I have reason from subsequent observations to conclude, in all, is covered with moving cilia; 3d, That the property continues for some time in detached portions of the tissue, in which case the impulsion of the fluid takes place in the same direction with respect to the surface of the parts, as before their separation.

At that time I was not aware of any similar observations, excepting those of Dr Grant and others, on the Infusoria and Zoophytes; but I have since found, that in some of the facts I have been anticipated by previous observers. The currents on the gills of the tadpole, and larva of the salamander, were previously described by Steinbuch.† His work, which I first saw in the library of Professor Rudolphi, in Berlin, in 1831, seems to have been little known, at least his discovery did not attract the attention it deserved; and, with one or two exceptions, it seems to have escaped the notice not only of general physiological writers, but even of those who have specially written on the development of the Batrachia, on the continent as well as in this country. Observations similar to those of Steinbuch seem, however, to have been made by

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* Edinburgh Medical and Surgical Journal, vol. xxxiv.

† Analecten neuer Beobachtungen und Untersuchungen für die Naturkunde. Fürth. 1802.

Huschke; and they have more recently (1832) been repeated by John Müller * and Raspail.† Again, I find that some of the facts respecting the mollusca have been noticed in the mussel, and two or three other species, by Leeuwenhoek, Baker, Hales, Erman, Treviranus, Gruithuisen, Huschke, Baer, and Raspail. These authors, however, have for the most part observed the phenomena but partially, and interpreted them erroneously, excepting Huschke, if I may judge from the brief reference to his observations on the mussel and salamander, in Burdach's Physiologie ‡—for I have not seen his original paper §—and Gruithuisen,‖ who discovered the cilia and currents in a species of fresh-water snail, and took a correct view of their nature and use. Subsequently, observations have been published on the mussel by Carus ¶ and Guillot,** in 1831, and on the ascidia by Mr Lister.†† The singular rotatory motion of the embryo of mollusca within the egg, described by Swammerdam, Leeuwenhoek, Baker, Baster, Stiebel, Hugi, Carus, and Grant, had also been referred to the same class of phenomena; and Dr Grant first demonstrated the existence of cilia as the organs by which the motion within the ovum is produced, though he seems not to have been aware of its connexion with the respiratory process.

Having at the time mentioned ascertained the existence of the motion in the batrachia, the principal tribes of mollusca, the annelida, and the actinia, and knowing that it had been found in infusory animalcules and zoophytes, I stated it as probable, on generalizing the results of the observations, that it prevailed very extensively in the animal kingdom. Accordingly, in prosecuting the inquiry with this view, when other avocations permitted, I have ascertained its existence in several additional instances. The account of these I reserve for an article which I have prepared for the Cyclopædia of Anatomy and Physiology, in which I shall endeavour to give a review of the whole subject, and where I hope, at the same time, to be able to do justice to the claims of others more fully, than can be looked for in this brief notice. I may, however, here state, in the mean time, that I have found the ciliary motion in several species of asterias and sea urchin, in the aphrodita, the serpula, and other genera of annelida, in addition to the one formerly examined; in the lymnea, planorbis, and other species of mollusca; and within the egg of the frog, as I had formerly observed it in the egg of the newt. In the asterias the ciliary motion exists, 1*st*, On the external surface; 2*d*, On the inner surface of the cavity containing the viscera, to which, as is well known, the water has access; its membranous lining, which is prolonged into the so-called respiratory tubes, and reflected on the cœca, exhibiting the phenomenon also in these situations; 3*d*, On the internal surface of the stomach and cœca; and, 4*th*, Within the tubular feet. In the aphrodita, I found it, 1*st*, On the external surface of the intestine and cœca, and on the lining membrane of the dorsal cells in which the cœca are lodged; and 2*d*, Within the intestine and cœca, on their internal surface. A careful examination of various species of actiniæ also shewed a striking correspondence between the phenomena, as exhibited in these animals and the two last mentioned; and it may be remarked, that the appearances in all these three cases, as also in the echinus, tend to support the views of those physiologists, who conceive that the digestive organs of these animals also exercise a respiratory function. I have now also succeeded in detecting cilia on the surfaces producing the currents in all instances except the sponge, even in the larva of the batrachian reptiles, though in some cases these organs are excessively minute, requiring a magnifying power of 300 diameters, and consequently considerable nicety of manipulation, to render them visible.

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* Burdach, Physiologie als Erfahrungs-wissenschaft. Band iv. p. 434.

† Chimie Organique, 1833, p. 250.

‡ Loc. cit.

§ In the Isis for 1826.

‖ Nova Acta Naturæ Curiosorum, vol. x. p. 437.

¶ Nova Acta Nat. Cur. vol. xvi. 1832.

** Journal de Physiologie, tome xi.

†† Phil. Trans. 1834, p. 378.

In suggesting the probability of the existence of this motion in warm-blooded animals, as a provision for impelling fluids along the surface of canals or cavities, independently of any muscular contraction of their parietes, I mentioned also that I had sought for the phenomenon, but without success, during the process of incubation of the chick. I have since then repeated these inquiries respecting the chick, but still without being able to detect the motion. It will be seen that Messrs Purkinje and Valentin were led to make their interesting discovery, by accidentally noticing the ciliary motion in the fallopian tube of a rabbit, which observation they have followed up with an extensive and well directed inquiry. Their memoir has been already made known in this country in the number of the Dublin Journal of Medical and Chemical Science for May 1835, through which I have to acknowledge having first obtained notice of it. The original is in the fifth number of *Müller's Archiv für Anatomie, &c. for* 1834. A translation of it here follows.

Discovery of Continual Vibratory Motions produced by Cilia, as a general Phenomenon in Reptiles, Birds, and Mammiferous Animals. By Professor PURKINJE and Dr VALENTIN, of Breslaw.

THE remarkable property possessed by parts of certain animals, of exciting currents in the medium surrounding them, which is generally fluid, has justly attracted the attention of naturalists, and given rise to numerous observations. The phenomenon was first discovered in the Infusoria, being easily recognizable in the rotatory apparatus of some of these creatures. Similar appearances were at a later period observed in mussels by Erman, Von Baer, Carus, and others; and the curious revolving motion of the embryo within the ovum, which is easily perceived in these animals, has been correctly referred to the same cause. Observations of the same kind were also made in many other avertebrated animals. The first discovery of any thing similar in the Vertebrata is due to Steinbuch, who observed the phenomenon on the gills of the larvæ of Batrachian Reptiles, although his description does not altogether correspond with nature. Carus, Hugi, E. H. Weber, Stiebel, John Müller, and other naturalists, have furnished particular contributions to the general collection of facts. We beg, however, to refer such as are desirous of making themselves acquainted with the subject, as far as it has been hitherto known, to an article by Sharpey in Froriep's Notizen, No. 618.

As one of us was engaged in the beginning of last spring, in examining a rabbit which had been impregnated three days previously, with the view of finding ova in the Fallopian tube, he perceived with the microscope, small portions of the mucous membrane of the tube moving briskly, and whirling round their axis. The other confirmed the truth of this observation, and at the same time recognised the appearance as an instance of vibratory motion. The whole uterus and the internal organs of generation generally were now diligently searched, and these motions were discovered throughout their entire extent; being nowhere absent, though of very different degrees of intensity at different places. They were especially brisk in the tubes, less so in the cornua of the uterus, still less in the conjoined parts of that organ, most lively and rapid of all on its swollen and dark red lips, and of considerable strength in the vagina. The next step which naturally suggested itself was to examine the oviduct of a bird immediately after the passage of an egg; and on making the trial we found, according to expectation, most lively and incessant vibratory motions along the whole oviduct. Animals of the same kind were then submitted to examination in the unimpregnated state, and in these also, as well as in the Amphibia, which were afterwards examined, we found a confirmation of our interesting and remarkable discovery. This naturally led us to search for the phenomenon in other parts of the animal body, and we thus arrived at results, of which we here in the mean time communicate the most important.

a. *Parts of the Body, and Classes of Animals, in which the Vibratory Motions exist.*

According to the numerous experiments we have hitherto made, the vibratory motion occurs only in two systems of organs, viz. in the sexual organs of the female, and in the organs of respiration; it is general over all parts of the internal surface of these organs, in mammalia, birds, and reptiles. No trace of it can be found in any part of the intestinal canal of vertebrated animals; even in the avertebrata we know of but one example of its existence on the intestinal membrane, and this is not a perfect one. The instance alluded to is the river mussel; the intestine of this animal presents a longitudinal fleshy ridge on its inner surface, on which the vibratory motion is visible, but the motion is confined to the elevation being wanting on the rest of the tube, a circumstance seeming to indicate that the part in question is connected with the generative function.* We have as yet been equally unable to discover the phenomenon in the male generative organs, the gall-bladder and ducts, the excretory ducts of glands, the urinary passages, the cerebral and spinal arachnoid membrane, the internal surface of the bloodvessels, the surface of the blood-globules, the membranes of the ovum and envelopes of the fœtus, and the skin.

In Amphibia, such as serpents, lizards, and the like, in birds and in mammiferous animals, the mucous membrane of the oviduct exhibits the vibratory motion throughout its entire extent, both in the impregnated and unimpregnated state, and the smallest portion, when examined with the precautions to be afterwards prescribed, is sufficient to show it. The same is the case with the mucous membrane of the respiratory passages, from its commencement to its termination; in so much that the presence of the property in question may serve as a sure test of the system to which the parts exhibiting it belong. In the mammalia the motion occurs over the whole lining membrane of the windpipe and its branches, extending to the smallest divisions which admit of investigation, but no trace of it can be found in the glottis and its ligaments, nor in the lining membrane of the mouth and pharynx. On the other hand, it is very conspicuous in the nose, and it ceases abruptly in the most striking manner at the limit between these parts. In reptiles, such as the salamander, however, in which the mouth is not merely an organ of deglutition, but serves also for respiration, the mucous membrane of the throat exhibits the motion in a very lively and conspicuous manner.

Should it be ultimately established that only the respiratory and genital membrane possess this property, it would afford a new fact in support of the analogy already held on many other grounds to subsist between them.

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* I have, however, as already mentioned, found the motion on the inner surface of the digestive organs of the Echinodermata, the Annelida, and Actinia, and it exists in the alimentary cavity of many polypi.—W. S.

We have, as already stated, discovered the general existence of this property in reptiles, birds, and mammalia; but, with all our endeavours, we have never been able to detect any trace of it in fishes. We examined, with this view, the feelers of the Silurus, the gills, the mucous membranes of the head, the coats of the intestines and swimming bladder, the kidneys with the ureter, and the skin of many other fishes. Nor was any thing of the kind visible in the embryo of Perca and Cyprinus, though examined in various early stages of its development.

b. *Method of Investigation.*

As the vibratory motions occur on the whole surface of the mucous membrane, in order to perceive them it is only necessary to obtain a view of the membrane under a sufficiently high magnifying power. In parts which have long cilia, such as the commencement of the oviduct of birds, this may be accomplished by simply spreading out the portion of membrane on the object-plate of the microscope, covering it with water, and viewing it with the requisite power of the instrument. But it is in general necessary to employ the following manipulation, in order to perceive the phenomenon with certainty.

The animal must be examined immediately after death. A small piece is to be cut out from the membrane to be observed, with a pair of fine curved eye-scissors, and folded on itself, in such a manner that the edge of the fold shall be formed by the free surface of the membrane, its adhering surface being now inside and in contact with itself. The object is then to be placed in the *microtomic compressor*,* with a little water, and gently pressed, until the folded edge is brought clearly into view under the microscope. Even this simple preparation is sufficient to exhibit the phenomenon in great beauty; but in order to render it still more conspicuous, a fluid is to be added, holding small particles of some substance in suspension. Nothing answers this purpose better than the black pigment of the eye diffused in water; but the less experienced observer must be cautious, when this is used to avoid being deceived by the molecular motion described by Brown, which the particles of pigment exhibit in a remarkable degree. Blood sufficiently diluted may also be employed for the same purpose. The current of these particles along the margin of the fold is so strong as to strike the most unpractised eye.

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* An instrument contrived or improved by Professor Purkinje, by which two parallel glass plates can be gradually approximated, so as to compress a soft object under the microscope to any required degree.

Some additional precautions are, in certain cases, rendered necessary by the nature of the tissue. It is in all cases absolutely necessary that the mucous membrane alone should be employed. Hence no portion of muscular coat, or of bronchial cartilage, must be left adhering to it, otherwise the observation is not merely impeded, but entirely frustrated.

On account of the peculiar structure of the lungs of reptiles, the following circumstance must be attended to in their examination. The interior of these organs presents a number of contiguous cells, whose parietes are united with one another in an elegantly reticulated manner; if a portion of such a lung be folded in the way prescribed, the mucous membrane of these cells will, for the most part, be still concealed, and its free surface will be exposed only in a few places; hence it is in such places alone that the motion is to be looked for.

c. *Nature and Character of the Vibratory Motions.*

The vibratory motions are extremely rapid motions which appear on the surface of the parts, the eye being scarcely able to follow their particular features, when viewed in their most vivid state. Wherever they occur, they observe, like the currents which they excite, a determinate direction; we have met with only one exception to this rule, namely, in the appendages of the gills of the river mussel, in which the movements went alternately in two different directions in a regular manner, the change of direction taking place every six or seven seconds. In all other cases, however, the movements, whatever their degree of vivacity, maintain invariably the same direction.

From our observations, it seems highly probable that the vibratory motions are always produced by cilia; for, as will be more particularly stated on another occasion, we have found evident traces of them even in the larvæ of the Batrachia. In the female genital, and in the respiratory organs of mammalia, birds, and reptiles, the cilia cannot be mistaken. They are most conspicuous in the oviduct of birds and serpents, less so in that of mammalia, and still less in the mouth of the salamander. While the motion is swift, the cilia can be distinguished only by a practised eye, but when it relaxes, they can be perceived rising and falling like oars, till at last, when all movement has ceased, they stand out like stakes from the edge of the folded membrane. Their figure can then be distinctly seen, tapering from the base to their excessively fine and delicate point; their substance is clear, without any appearance of granular structure, and their consistence is exceedingly soft and tender, so that they are very easily destroyed.

No extrinsic agency has any influence over the vibratory motions in the three higher classes of vertebrata. The contraction of the subjacent muscular textures, as in the uterus of mammalia, merely hinders the observation of the phenomenon, which becomes clearly visible on removing them. Animal heat also exerts no special influence; the vibrations being equally brisk in parts that have long been cold, or which have lain in cold water, as in those which are quite warm.

In the same animals also, the vibratory motion is not only of sufficient force to propel small particles immediately adjacent to the surface, but is such also, that, by its means, small portions of the mucous membrane, when detached, move themselves through the fluid. This last effect is striking enough, but it is not seen to such a degree as we have had occasion to witness it in detached portions of the mussel. Although the vibratory motion of the latter is by no means of greater rapidity, force, and intensity than in the vertebrata, yet it possesses far greater durability; since, according to our own experiments, it continues to exist with undiminished vigour in mussels which are in a semi-putrid, softened, and macerated condition. In the vertebrata, on the other hand, the addition of a drop of acid or alkaline solution to the fluid instantly arrests the motion.*

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* I have found that the motion on the gills of batrachian larvæ continues in water deprived of air by boiling, or impregnated with carbonic acid, or saturated with muriate of morphia. In the sea mussel, it is instantaneously stopped by fresh water.—W. S.

The duration of the vibrations is very different in different cases. In the oviduct of birds, it persists for about half an hour after death; in the corresponding part of mammalia about twenty minutes; and in both it lasts about half as long again in the respiratory organs. Indeed, we found it very vigorous in the nose of a rabbit that had been dead two hours.

We have already remarked how different the motion was in different parts of the sexual organs of a rabbit that had been three days impregnated. In gravid animals of this kind, the motion exists on all the internal surface of the organs, excepting those parts which adhere to the chorion or external envelope of the fœtuses. It is very strong in the small intervals, where the surface of the membrane is free. We sought for it in vain, however, in the uterus of a bitch which had pupped the day before, and from whose organs a large quantity of liquid and diluted blood was discharged. We reserve this point for our future investigations. We have also endeavoured to ascertain whether the vibratory motions are increased or diminished in intensity by inflammation of the genital and respiratory organs. There is one circumstance that seems to favour the first supposition, viz. that in a female rabbit killed a few days after the access of the male, the motion was strongest in the lips of the uterus, which were affected in a manner resembling inflammation.

Although the vibratory motion is to be regarded more as a general morphological phenomenon, yet we cannot entirely overlook its particular uses. For, by its means, the secretions of those mucous membranes on which it occurs may be conveyed onwards, and many singular phenomena may perhaps be accounted for in this way. Thus, for instance, when the bronchial mucus accumulates during a long uninterrupted sleep, and is afterwards discharged, we do not bring it up from the interior of the lungs, but only from the larynx or top of the windpipe. But we refrain from pointing out further applications, that we may avoid the field of mere hypothesis, which is here so tempting.

Although we here speak of vibratory motions *, the term is

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* *Flimmerbewegungen*, of which word " vibratory motions" is not a literal

not to be understood in the sense in which it has occasionally been employed of late, as, for example, when applied to a peculiar appearance in the blood. On the contrary, we use it to denote a phenomenon as well defined and regular as any in nature. It no doubt sounds strange that hairs should be found on the mucous membrane of the respiratory passages, or that hairs or cilia should exist at all within the body, especially to such an extent; but in proof of it we can confidently refer every one to his own observations, which are here easily made. We must frankly own, however, that, in order to form a competent judgment, it is requisite to command a clear magnifying power of from three to four hundred diameters. Whoever is provided with such means, and attends to the precautions above prescribed, will be able easily to perceive and to admire one of the most beautiful phenomena in nature.

We hope very soon to make known the entire of our observations on the vibratory motions in the animal kingdom generally, in a special work which we may perhaps be able to submit to the public in the beginning of next year.

***Breslaw**, 4th July* **1834.**

Additional Observations and Experiments by Dr Sharpey.

Since learning of the interesting discovery of Messrs Purkinje and Valentin, I have repeated several of their observations; and, as in a subject of this sort, it is important to be able to compare the results obtained by different persons; I beg to subjoin a statement of those I have hitherto met with; more especially as I have in some cases ascertained the direction in which the propulsion of matters along the surface takes place, a circumstance not mentioned by the authors in their present preliminary memoir.

On examining one or more examples of mammalia, birds and reptiles, I have been able satisfactorily to perceive the ciliary motion in all the three classes. The cilia, indeed, are very minute, and their presence is indicated rather by the oscillatory motion which they conjunctly exhibit, than by their individual

translation; but the term "vibration" has been before applied to the motion of the cilia, and is used also by the authors themselves as synonymous with the other.

appearance; but they are not smaller than I had formerly seen them in several instances, and here also their motion is equally characteristic as in those cases in which they are larger. For the most part I used a doublet lens of $\frac{1}{35}$ of an inch focus, though the phenomenon could be sometimes seen with a lens of $\frac{1}{12}$ of an inch. The piece of membrane to be examined was folded as directed, wetted with water, and covered with a thin plate of mica, which spreads out the object, disposes the water suitably at the edge of the fold, at the same time preventing its evaporation, and obviates the risk of the lens touching the fluid when a very high power is employed.

The mammiferous animals I made use of were rabbits. In these I first perceived the phenomenon on the mucous membrane of the trachea, nostrils and maxillary sinus. The readiest way of seeing it in the windpipe, I found, was to cut out a portion of the posterior membranous part of the tube where the cartilages are wanting, as this can be readily folded for examination with the microscope. The ciliary motion was particularly strong in the nasal cavities, and, as noticed by Purkinje, continued longer in this than in other situations. It seemed to me more distinct on the membrane covering the inferior turbinated bone, and within the maxillary sinus, less so on the septum of the nose. The course of the impulsion could be readily determined on the inferior turbinated bone; on placing the parts in tepid water, and letting fall some charcoal powder on their surface, the particles were slowly carried from behind forwards, following the direction of the projecting laminæ of the bone *a a*, Fig. 1, Plate III., where the arrows indicate the direction in which the particles were conveyed. On breaking open the maxillary sinus and trying it in the same way, the impulsion seemed to be directed to the back part of the cavity, and it is there that its opening is situated, which is concealed from view in the figure at *b*, by the ethmoidal spongy bones.

I made several unsuccessful trials before I could distinguish the ciliary motion in the generative organs of the rabbit. At length, however, I perceived it very distinctly and satisfactorily in the Fallopian tube. Two of the unsuccessful attempts were made on rabbits three or four days after impregnation, as appeared from small ova which were found in the uterus.

I next examined the same parts in pigeons. On the mucous lining of the wind-pipe and nose, the ciliary motion was found as in the rabbit; I sought for it also on the membranous parietes of the abdominal air-cells, but without success. I could not perceive it in the oviduct, but I lay no stress on this negative result, as the only two examples in which I was able to examine this part were young pigeons, and as I met with repeated failures before detecting the motion in the Fallopian tube of the rabbit.

The reptiles examined were the water salamander or newt, the toad, and the frog. In all the three I found the ciliary motion very distinct in the mouth and gullet; in none of them could I find it in the lungs, notwithstanding very careful trials. In regard to the oviduct, I have as yet been able to examine it only in the newt, and hitherto without success; for although I could indistinctly perceive something like the motion on the edges of its superior orifice, I could not detect it on the internal surface of the tube, on carefully inspecting it in various parts of its long extent, and in several specimens.

The ciliary motion within the throat of the batrachia is very remarkable. It occurs from the opening of the mouth all the way to the termination of the œsophagus. The direction of the impulsion is easily ascertained by means of particles of charcoal, and I have in this way taken some pains to trace it exactly. Figures 2 and 3 represent the parts where the motion exists, and the direction of the impulsion in the newt, and figures 4 and 5 the same in the toad, the appearances in the frog being entirely similar. The arrows point out the situation and direction of the currents. In Figs. 2 and 4, *a* is the lower jaw, detached from the head, *b* the tongue, *c* the glottis, *d* the œsophagus, cut off from the head (at *g*, *g*, Figs. 3 and 5), and laid open from above, *e* the stomach, and *f*, *f*, the lungs. The general course of the currents is longitudinal, beginning at the symphysis of the lower jaw, and extending to the lower end of the œsophagus, where they terminate abruptly in a very striking manner. At particular parts they, for the most part, follow the direction of the plaits of the lining membrane. Figures 3 and 5 represent the head and the roof of the mouth, from which the lower jaw has been separated On this part of the mouth also the general course is longitudinal, from before backwards ; at the nostrils, *h*, *h*, especially in the newt, the particles are drawn in at one edge of the opening and issue at the other, as indicated in the outline of Fig. 3.

I do not hazard any opinion on the question, whether this provision in the mouth and gullet of the batrachia serves merely to convey the secretions of these parts to the stomach, or whether it may not also be specially connected with respiration. As in some measure connected with the present subject, however, I may here take the opportunity of mentioning a fact respecting the newt, which I do not remember to have seen noticed. The experiments of Spallanzani, and more especially those of Edwards, have shown that the batrachian reptiles not only breathe the air of the atmosphere, but can live exclusively by means of the air contained in water. In the latter case, the air of the water is supposed to act on the skin, since in frogs immersed in water, Spallanzani never, and Edwards only in a very few instances, perceived any movement of deglutition by which the fluid could be conveyed into the lungs. I have, however, repeatedly observed that the newt, when submersed at the present season of the year, regularly inhales water by the nostrils, and discharges it by the mouth at least twelve or fifteen times in a minute. The water in this case certainly enters the throat. Whether it reaches the lungs, or whether, on the contrary supposition, its entrance merely into the mouth and pharynx forms part of the respiratory process, are questions which remain to be solved.

In recapitulation, I may briefly state, first, that the repetition of Purkinje and Valentin's observations to the extent related, confirms their discovery of the ciliary motion in mammalia, birds, and perfect reptiles, viz. in the air-passages and Fallopian tubes of mammalia, in the air-passages of birds, and in the mouth and throat of the batrachia ; the negative observations respecting the oviduct of the bird being inconclusive. Secondly, that these additional observations have also shewn, in certain cases, the direction in which matters are impelled along the surface.

3. Alva Street, *May* 1835.

BRITISH MUSEUM
NATURAL HISTORY.

Fig. 1.

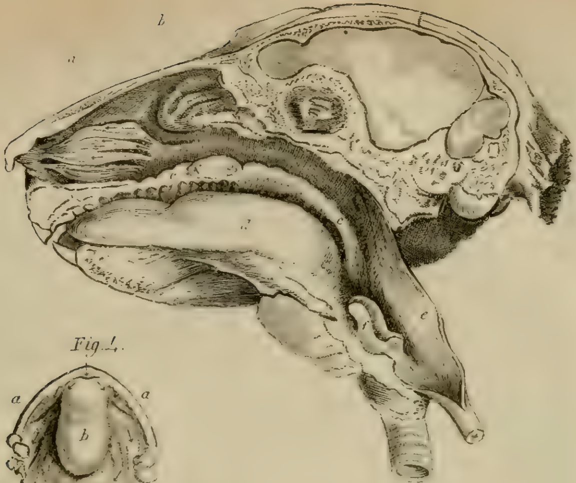


Fig. 4.

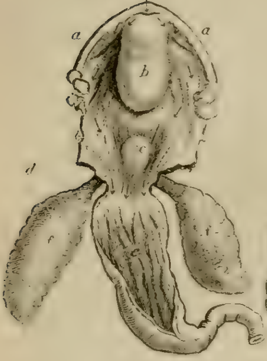


Fig. 5.

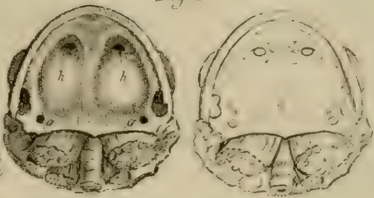


Fig. 2.

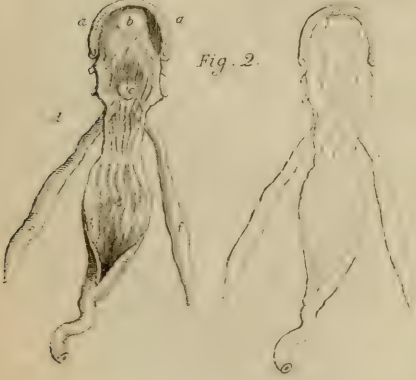
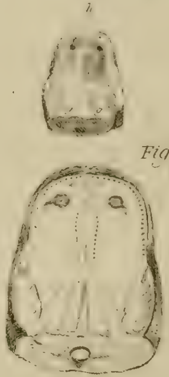


Fig 3.



E. Mitchell, Sc.

Explanation of Plate III.

Fig. 1. Right half of the head of a Rabbit.
- *a.* Inferior turbinated bone. The direction of the currents is indicated by the arrows.
- *b.* Superior turbinated bone.
- *c. c.* Palate.
- *d.* Tongue.
- *e.* Pharynx.
- *f.* Epiglottis and opening of the larynx.

Figs. 2 and 3 are from the Newt; Figs. 3 and 4 from the Toad. The letters refer to the same parts in both. On the accompanying outlines of which that in Fig. 3 is enlarged, the arrows indicate the situation and direction of the currents.
- *a. a.* Lower jaw detached from the upper.
- *b.* Tongue.
- *c.* Glottis.
- *d.* Gullet cut off from the head (at *g. g.* Figs. 3 and 5), and laid open above.
- *e.* Stomach.
- *f. f.* Lungs.

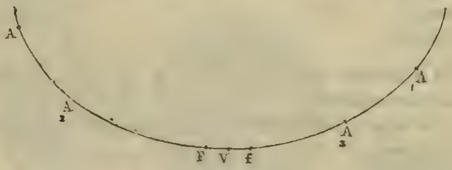
Figs. 3 and 5 represent the head and roof of the mouth, from which the lower jaw has been separated.
- *g. g.* Cut edge of the membrane where the pharynx and gullet were detached.
- *h. h.* Nostrils.

On the Manner in which Friction affects the Motions of Time-Keepers. By Edward Sang, Esq., Teacher of Mathematics, Edinburgh, Member of the Society of Arts, &c. Communicated by the Author.

In the usual method for determining the length of the seconds pendulum, the effect of friction is allowed to accumulate until a great part of the motion is destroyed. Yet no regular investigation, that I know of, has ever been given of the influence which this circumstance must exert upon the final result. The measurement of the length of the pendulum throws great light on one of the most difficult questions of physical astronomy, and at the same time furnishes a distinct standard to which the linear measures of different nations may be referred. As might have been expected, a subject of such vast importance has received the attention of the most distinguished analysts. Laplace has given the general equation of the curve which will produce isochronism in a medium whose resistance is a function of the first and second powers of the velocity, while Poisson has attempted the investigation when the resistance is supposed minute; but neither philosopher gives to the result a form suited to practical purposes.

For the purpose of illustrating the succeeding investigations, I shall first consider the motion of the cycloidal pendulum when affected by a constant friction.

Let V be the vertex of a cycloid, in which the motion of a body is retarded by a constant friction, acting in a direction tangent to the curve. The most convenient way for representing the friction is to assume a distance VF or V*f* along the curve, such that the tendency of the body to descend from F or *f* may be just equal to the friction. When the body is at A, its tendency to descend is, from the nature of the cycloid, proportional to VA, while the friction is proportional to VF, wherefore the surplus tendency to descend must be proportional to FA. When the body has descended and reached the other side of F between F and V, V*a* the tendency down is less than the friction VF, and therefore the motion is retarded as if by a pressure F*a*. And again, when the mobile reaches the other side of V also, both the friction and gravitation tend to retard its motion, so that the retarding pressure is still proportional to its distance from F. It thus appears that the body oscillates about F just as it would have oscillated, at equal distances, about V, had there been no friction.



The time of oscillation in the cycloid is, then, not altered by a constant friction.

Supposing A to be the point at which the motion commenced, the body would descend with increasing velocity until it reached the point F; there its motion would begin to be retarded, and it would finally be arrested at a distance $FA_{,}$ equal to FA. As soon as the returning oscillation is commenced, the friction changes its direction, the vertex of the motion is transferred to f, and the distance fA_2 measured equal to $fA_{,}$ defines the limit of the oscillation. Denoting by A the extreme distance at the beginning, and by A_n that at the end of n oscillations, also putting φ for the arc VF, we have

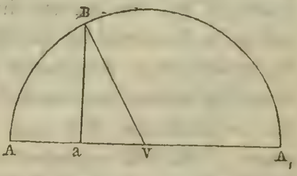
$$A - A_n = 2n\varphi,$$

as is evident from the above construction: In words, *The extents of the oscillations decrease in arithmetical progression.*

As the motion of the cycloidal pendulum represents isochronous motions in general, it follows that a constant friction, while it diminishes the extents of the oscillations in arithmetical progression, leaves unaltered the time in which each oscillation is performed; and there would result this remarkable conclusion, that variations in the viscidity of the oil should not influence the rate of a chronometer. Such, in fact, would be the case were it not that the maintaining power supplies, at some part of each oscillation, the motion which has been consumed by the friction during the rest of it.

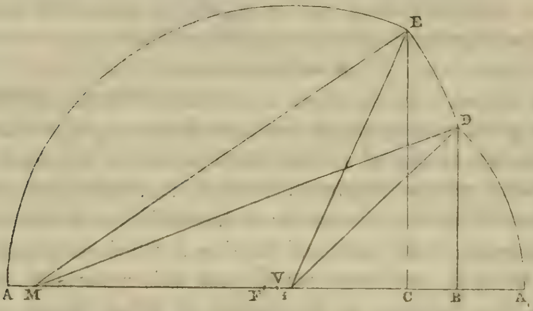
Before proceeding to consider the effect of the stroke of the escapement, I may recal to the recollection of those who have not lately been examining this subject, the well-known law of isochronous oscillations.

Conceiving the path of the oscillating body to be expanded into a straight line, let $AVA_{,}$ be the entire extent of the motion, and suppose a to be an intermediate position. Having described a semicircle on $AA_{,}$, erected the perpendicular aB, and joined VB, aB will represent the velocity acquired at a, while the angle AVB will be proportional to the time of description of the path Aa, and the supplement BVA to the time of describing $aA_{,}$.



This being premised, I proceed to consider the motion of the watch-balance, when maintained by what is technically called the *chronometer escapement.* Disregarding the minute resistances experienced in the repassing of the slender spring attached to the detent lever, and in lifting the detent itself, the action of this escapement may be thus described:

If V represent the centre of the oscillations, VF = V*f* the amount of friction, and A the point from which the oscillation commences, the elasticity of the spring, acting with an intensity proportional to VA, but diminished by the friction VF, will cause the balance to describe an oscillation round F as a vertex, and carry it to the distance FA, = FA on the other side. The time of this oscillation may be represented by the half revolution AFA,.



When the balance begins to return from A,, it encounters a friction V*f*, and would clearly fall short of A, by a distance double of F*f*. To prevent this, to restore the motion destroyed and to be destroyed by the friction, the maintaining power of the movement is introduced at the instant, we shall suppose, when the balance has reached the position B. Having erected the perpendicular BD, and cut it by the arc A,B from the centre *f*, BD will represent the velocity at B, and the angle A,*f*D the time occupied in the description of the path A,B. Representing by the line *f*M the intensity of the pressure communicated by the impulse tooth, M will clearly be the vertex of the minute part of an oscillation performed by the balance while under the influence of the maintaining power; the ordinates of an arc described from M, with the radius MD, would clearly represent the velocities at different points of that portion of the oscillation. Supposing C to be the point at which the maintaining pressure ceases to act, CE would be the velocity which the balance has when left to the agency of the friction and of the balance spring. The remainder of the oscillation has *f* for its vertex, so that, since the motion is to cease at A, the distance *f*E must be equal to *f*A. The point E, then, is determined by the intersection of the arc described from M, with the radius MD, and that described from *f*, with the radius *f*A. The perpendicular drawn from E thus indicates the termination of the arc of impulsion.

The extent of the arc of impulsion can be determined from the principle of virtual velocities; and must be the fourth proportional to the *maintaining pressure*, the *friction*, and the *entire extent* (twice AA,) *of the oscillation*.* The time of the description of BC will be represented by the angle DME, while that of the description of CA will be measured by E*f*A.

It thus appears that the entire time of return from A, to A is represented by the sum of the three angles A,*f*D, DME, and E*f*A. But, had the maintaining pressure not acted, that time would have been represented by a half revolution, or by the sum of the three angles A*f*D, D*f*E, and E*f*A; whence the acceleration caused by the maintaining power is propor-

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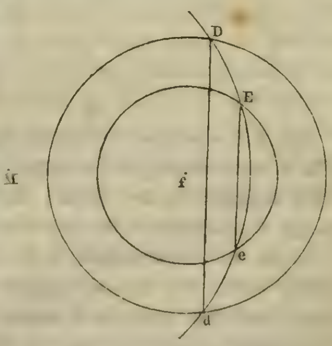
* This leads to the following beautiful proposition, which I do not recollect of ever having seen before:—

PORISM.

If from any eccentric point M an arc be described cutting two concentric circles, twice the rectangle under the excentricity M*f*, and the distance between the chords D*d* and E*e* of the intercepted arcs, is equivalent to the difference between the squares of the radii of the concentric circles, and that independently of the radius of the secant circle.



tional to the difference between the angles $\mathrm{D}f\mathrm{E}$ and DME, or what is the same thing, to $\mathrm{ME}f - \mathrm{MD}f$.

Having obtained this simple method for determining the influence of the maintaining pressure on the rate of the chronometer, I proceed to solve the interesting problem,

To place the arc of impulsion so that the time of the oscillation may not be altered by the stroke of the escapement.

The question at once reduces itself to this; the points A, M, f and $\mathrm{A}_{,}$ being given, to find the length of the radius MD, in order that the angle $\mathrm{ME}f$ may be equal to $\mathrm{MD}f$. Now, if these angles be equal to each other, their cosines will also be so, whence

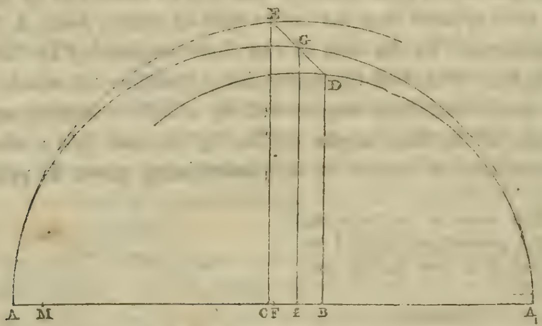
$$\mathrm{ME}^2 + \mathrm{E}f - \mathrm{M}f^2 : \mathrm{MD}^2 + \mathrm{D}f^2 - \mathrm{M}f^2 :: 2\mathrm{ME}\cdot\mathrm{E}f : 2\mathrm{MD}\cdot\mathrm{D}f \quad \text{Or}$$

$$\mathrm{MD}^2 + \mathrm{A}f^2 - \mathrm{M}f^2 : \mathrm{MD}^2 + \mathrm{A}_{,}f^2 - \mathrm{M}f :: \mathrm{A}f : \mathrm{A}_{,}f \quad \text{From which}$$

by division $$\mathrm{MD}^2 + \mathrm{A}_{,}f^2 - \mathrm{M}f^2 : \mathrm{A}f^2 - \mathrm{A}_{,}f^2 :: \mathrm{A}_{,}f : \mathrm{A}f - \mathrm{A}_{,}f \quad \text{Or}$$

$$\mathrm{MD}^2 - \mathrm{M}f^2 = \mathrm{A}f\cdot f\mathrm{A}_{,}\,.$$

Hence the following very simple construction:



On AA, describe the semicircle, and erect the perpendicular $f\mathrm{G}$ so as to obtain $f\mathrm{G}^2 = \mathrm{A}f\cdot f\mathrm{A}_{,}$: and from M with the radius MG describe the arc EGD; the perpendiculars EC, DB, will define the position of the arc of impulsion.

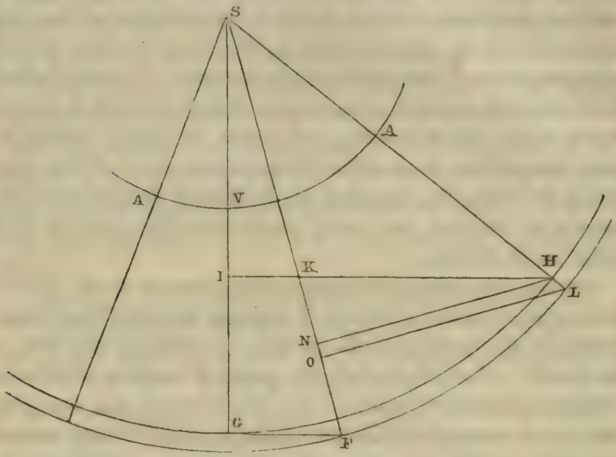
From this construction, it is clear that the point f is invariably within the arc of impulse corresponding to no acceleration; and that when, as in actual practice, the friction is small, the proper position for that arc is just a little before the centre of the oscillation. When the arc BC is removed nearer A, the escapement accelerates; but when it is removed nearer A, the escapement retards the rate of the chronometer.

It is almost unnecessary to remark, that the foregoing propositions enable us either to compute the influence of such an escapement on the rate of a clock pendulum, or to place the arc of impulse so that that influence may be zero. It would indeed be worth while to try to what degree of precision such a process would enable us to attain; seeing that we would thereby be saved the tedious observation of the coincidences of one pendulum with another, and that we would reap the additional advantage of retaining the arc of oscillation nearly uniform for a great length of time.

Another very important question here presents itself: To determine the position of the arc of impulse, in order that a slight change in the viscidity of the oil may not influence the rate of the watch. The solution of this question requires merely an ordinary operation in maxima, but unfortunately that operation leads to an enormously complex equation, which I have not yet been able to reduce to manageable dimensions. I shall give the investigation as it at present stands, if no better can be made out, in the succeeding Number of this Journal.

The investigation into the motions of the common or circular pendulum, present difficulties of an order higher than those we have yet encountered. The friction on the knife edge is proportional to the pressure of that edge against the plate on which it rests. Now, the pressure diminishes as the cosine of the inclination, but increases with the centrifugal force, so that one part of the inquiry must refer to the retardation caused by the statical part of the friction; another to the effect of a friction proportional to the square of the velocity. The latter part of the inquiry may evidently be engrossed into the computation of the effects of the air's resistance; and I shall, therefore, confine my attention to the first part of it.

Let SV represent the vertical position of a pendulum, produce it till SG represent gravity, and erect the perpendicular GF to represent the value of the friction when the pendulum presses the knife edge against the plate with its whole weight. Suppose the pendulum to be turned aside into the position SA, produce SA to meet the arc described from S with the radius SG, and draw the horizontal line HKI; HI will be the tendency down the arc, and SI will be the pressure of the knife



edge against the plate: wherefore, since the friction is proportional to the pressure, IK will represent the friction in the position SA, leaving the surplus HK to accelerate the descent of the pendulum. From S with the radius SF describe an arc, produce SH to meet it in L, and draw HN, LO, perpendicular to SF. The trigon KHN is clearly similar to FSG, whence

KH : HN : : FS : SG : : LS : SH : : LO : HN

to that HK is equal to LO.

Now, if gravity, instead of acting in the direction of and with the intensity SG, were to act in the direction of and with the intensity SF, LO would just be the tendency of the pendulum to descend from the point A; and, therefore, the first oscillation will be performed equally on each side of the line SF, and will be accelerated just as if gravitation were increased from SG to SF. The next oscillation would be performed equally on each side of a line, making with SG an angle equal to FSG, and thus the arcs of oscillation would decrease in arithmetical progression; the extreme distances from the vertical line being diminished at each oscillation by twice the angle GSF.

If A denote the inclination at the beginning, A_n that at the end of n oscillations, and φ the angle GSF, we have

$$A - A_n = 2n\varphi$$

and thus the value of the angle φ can be obtained by direct experiment.

From this proposition, it is clear that the friction does accelerate the oscillations of a pendulum; but then, in every well made instrument, the value of the angle GSF is so exceedingly small, that its secant will not differ, by any appreciable quantity, from the radius, and thus the usual deductions are left intact.

Having now ascertained the extents of the oscillations, it remains to compute the time during which any given number of them will be performed. For this purpose I must have recourse to the integral calculus. The algebraic method would indeed have easily conducted us to the conclusions already arrived at; but from a partiality to the perspicuity of actual representation, as well as from a wish to render this important subject as generally known as possible, the geometric form has been preferred.

Let l be the length of the pendulum, a the angle which at any time it makes with the vertical line, A the value of a at the commencement of the oscillation, and φ the angle of friction, we have

$$\begin{aligned}\frac{\delta v}{\delta t} &= -g\{\sin a - \cos a \tan\varphi\} \\ &= -g \sec\varphi\,\{\sin a \cos\varphi - \cos a \sin\varphi\} \\ &= -g \sec\varphi \,.\, \sin(a-\varphi)\end{aligned}$$

But $v = l\dfrac{\delta a}{\delta t}$ whence

$$v\,\delta v = -gl\sec\varphi \,.\, \sin(a-\varphi)\,\delta a$$

Or integrating

$$v^2 = l^2\left(\frac{\delta a}{\delta t}\right)^2 = -2gl\sec\varphi \,.\, \text{ver}(a-\varphi) + C$$

Now the velocity is zero when $a = A$ whence

$$\begin{aligned}\left(\frac{\delta a}{\delta t}\right)^2 &= \frac{2g\sec\varphi}{l}\left\{\text{ver}(A-\varphi) - \text{ver}(a-\varphi)\right\} \\ &= \frac{4g\sec\varphi}{l}\left\{\left(\frac{A-\varphi}{2}\right)^2 - \left(\sin\frac{a-\varphi}{2}\right)^2\right\} \quad \text{Or}\end{aligned}$$

$$\delta t = \frac{1}{2}\sqrt{\frac{l}{g\sec\varphi}}\left\{\left(\sin\frac{A-\varphi}{2}\right)^2 - \left(\sin\frac{a-\varphi}{2}\right)^2\right\}^{-\frac{1}{2}} \,.\, \delta a$$

The integration now needed for the determination of t, can be performed in a variety of ways, fitted for different species of inquiries: the usual substitution

$$\sin\frac{a-\varphi}{2}=\sin\frac{\mathrm{A}-\varphi}{2}\ .\ \sin e$$

best suits the present. Computing from this the value of ∂a in terms of ∂e, substituting and simplifying we obtain

$$\partial t=\sqrt{\frac{l}{g\sec\varphi}}\left\{1-\left(\sin\frac{\mathrm{A}-\varphi}{2}\right)^2\sin e^2\right\}^{-\frac{1}{2}}\partial e,$$

which expanded by the binomial theorem, becomes

$$\partial t=\sqrt{\frac{l}{g\sec\varphi}}\left\{1+\frac{1}{2}\left(\sin\frac{\mathrm{A}-\varphi}{2}\right)^2\sin e^2+\frac{1.3}{2.4}\left(\sin\frac{\mathrm{A}-\varphi}{2}\right)^4\sin e^4+\text{etc}\right\}\partial e\ .$$

Each term of this series can be integrated by means of the formula

$$\int\sin Z^n\ \partial Z=\frac{n-1}{n}\int\sin^{n-2}\partial Z-\frac{1}{n}\cos Z\sin Z^{n-1}\ .$$

After the proper arrangements the value becomes

$$\begin{aligned}t\sqrt{\frac{g\sec\varphi}{l}}=e&\left\{1+\left(\frac{1}{2}\right)^2\left(\sin\frac{\mathrm{A}-\varphi}{2}\right)^3+\left(\frac{1.3}{2.4}\right)^2\left(\sin\frac{\mathrm{A}-\varphi}{2}\right)^4+\left(\frac{1.3.5}{2.4.6}\right)^2\left(\sin\frac{\mathrm{A}-\varphi}{2}\right)^6+\text{etc}\right\}\\ -\cos e\sin e&\left\{\left(\frac{1}{2}\right)^2\left(\sin\frac{\mathrm{A}-\varphi}{2}\right)^2+\left(\frac{1.3}{2.4}\right)^2\left(\sin\frac{\mathrm{A}-\varphi}{2}\right)^4+\left(\frac{1.3.5}{2.4.6}\right)^2\left(\sin\frac{\mathrm{A}-\varphi}{2}\right)^6+\text{etc}\right\}\\ -\frac{2}{3}\cos e\sin e^3&\left\{\left(\frac{1.3}{2.4}\right)^2\left(\sin\frac{\mathrm{A}-\varphi}{2}\right)^4+\left(\frac{1.3.5}{2.4.6}\right)^2\left(\sin\frac{\mathrm{A}-\varphi}{2}\right)^6+\text{etc}\right\}\\ -\frac{2.4}{3.5}\cos e\sin e^5&\left\{\left(\frac{1.3.5}{2.4.6}\right)^2\left(\sin\frac{\mathrm{A}-\varphi}{2}\right)^6+\text{etc}\right\}\\ &+\text{etc.}\end{aligned}$$

This formula enables us to compute the time of the arrival of the pendulum at any point in its path, the time being reckoned from the instant of maximum velocity. It would be easy, were it worth while, to shew how it could be applied to the computation of the influence of a given escapement on the rate of a clock. The preceding is analogous to that already given for watches. For the purpose of aiding these computations, the logarithms of the coefficients are given in Tables I. and II.

TABLE I. Logarithms of Coefficients $\left(\frac{1}{2}\right)^2$, &c.		TABLE II. Logarithms of Coefficients $\frac{2}{3}$, &c.	
Coefficient.	Logarithm.	Coefficient.	Logarithm.
$\left(\frac{1}{2}\right)^2$	9.39794 00087	$\frac{2}{3}$	9.82390 87409
$\left(\frac{1.3}{2.4}\right)^2$	9.14806 25355	$\frac{2.4}{3.5}$	9.72699 87279
$\left(\frac{1.3.5}{2.4.6}\right)^2$	8.98970 00434	$\frac{2.4.6}{3.5.7}$	9.66005 19383
$\left(\frac{1..7}{2...8}\right)^2$	8.87371 61494	$\frac{2...8}{3...9}$	9.60889 94159
$\left(\frac{1...9}{2..10}\right)^2$	8.78220 11683	$\frac{2..10}{3..11}$	9.56750 67307
$\left(\frac{1...11}{2..12}\right)^2$	8.70662 40465	$\frac{2..12}{3..13}$	9.53274 46244
$\left(\frac{1..13}{2..14}\right)^2$	8.64225 46798	$\frac{2..14}{3..15}$	9.50278 14011
$\left(\frac{1..15}{2..16}\right)^2$	8.58619 72326	$\frac{2..16}{3..17}$	9.47645 24623
$\left(\frac{1..17}{2..18}\right)^2$	8.53655 00651	$\frac{2..18}{3..19}$	9.45297 13665
$\left(\frac{1...19}{2..20}\right)^2$	8.49199 72757	$\frac{2..20}{3..21}$	9.43178 20674
$\left(\frac{1...21}{2..22}\right)^2$	8.45159 05035	$\frac{2..22}{3..23}$	9.41247 69122
$\left(\frac{1..23}{2-24}\right)^2$	8.41462 36921	$\frac{2..24}{3..25}$	9.39474 81453
$\left(\frac{1..25}{1-26}\right)^2$	8.38055 70135	$\frac{2..26}{3..27}$	9.37835 77291
$\left(\frac{1..27}{2..28}\right)^2$	8.34896 84792	$\frac{2..28}{3..29}$	9.36311 77625
$\left(\frac{1..29}{2-30}\right)^2$	8.31952 19655	$\frac{2..30}{3..31}$	9.34887 73234
$\left(\frac{1..31}{2..32}\right)^2$	8.29194 53965	$\frac{2..32}{3..33}$	9.33551 33618
$\left(\frac{1-33}{2-34}\right)^2$	8.26601 54422	$\frac{2..34}{3..35}$	9.32292 42345
$\left(\frac{1..35}{2..36}\right)^2$	8.24154 65294	$\frac{2..36}{3..37}$	9.31102 50112
$\left(\frac{1..37}{2..38}\right)^2$	8.21838 27843	$\frac{2-38}{3\ 39}$	9.29974 40008
$\left(\frac{1..39}{2..40}\right)^2$	8.19639 20157	$\frac{2..40}{3..41}$	9.28902 01354

The value $e = \frac{\pi}{2}$ corresponds to $a - \varphi = A - \varphi$, while $e = -\frac{\pi}{2}$ corresponds to $a - \varphi = -A + \varphi$; whence the entire time of the first oscillation is

$$T = \pi\sqrt{\frac{l}{g \sec \varphi}}\left\{1 + \left(\frac{1}{2}\right)^2\left(\sin\frac{A-\varphi}{2}\right)^2 + \left(\frac{1.3}{2.4}\right)^2\left(\sin\frac{A-\varphi}{2}\right)^4 + \text{etc}\right\}.$$

On supposing $A - \varphi$ exceedingly minute, the value of the above time becomes $\pi\sqrt{\frac{l}{g\sec\varphi}}$: but the time of the cycloidal oscillation, were there no friction, would be $\pi\sqrt{\frac{l}{g}}$, so that if τ denote the true time of a cycloidal oscillation, we have

$$T_1 = \tau\sqrt{\cos\varphi}\left\{1+\left(\frac{1}{2}\right)^2\left(\sin\frac{A-\varphi}{2}\right)^2+\text{etc}\right\}\ .$$

In the very same way we would find

$$T_2 = \tau\sqrt{\cos\varphi}\left\{1+\left(\frac{1}{2}\right)^2\left(\sin\frac{A-3\varphi}{2}\right)^2+\text{etc}\right\}\ \text{Or generally}$$

$$T_n = \tau\sqrt{\cos\varphi}\left\{1+\left(\frac{1}{2}\right)^2\left(\sin\frac{A-(2n-1)\varphi}{2}\right)^2+\text{etc}\right\}\ .$$

The method of determining the duration of a beat of the reversing pendulum, is to draw it aside, and, allowing it to oscillate freely, to contrast its motions with that of a pendulum kept going by clock work: the rate of the clock being known, and the entire time $T_1 + T_2 + \ldots . T_n$ of a given number of oscillations of the experimental pendulum, the average $\frac{T_1 + T_2 + \ldots . T_n}{n}$ gives the first approximation to the duration of the beat. Denoting this average by T, we have

$$\begin{aligned}\frac{nT}{\tau\sqrt{\cos\varphi}} = n + \left(\frac{1}{2}\right)^2 &\left\{\left(\sin\frac{A-\varphi}{2}\right)^2 + \ldots\ldots \left(\sin\frac{A-(2n-1)\varphi}{2}\right)^2\right\} \\ +\left(\frac{1.3}{2.4}\right)^2 &\left\{\left(\sin\frac{A-\varphi}{2}\right)^4 + \ldots\ldots \left(\sin\frac{A-(2n-1)\varphi}{2}\right)^4\right\} \\ &+\text{etc}\ .\end{aligned}$$

And, to complete the investigation, it only remains to obtain the sums of the series indicated in the second member of the equation, in such a form as to give a ready computation.

In all practical cases, the value of φ is exceedingly small, and the number n very large; so that φ being regarded as the differential of $\frac{A}{2}$, and $n\varphi$ being the difference between $\frac{1}{2}A$ and $\frac{1}{2}A_n$, we have

$$\begin{aligned}\frac{\frac{1}{2}(A-A_n)T}{\tau\sqrt{\cos\varphi}} = \frac{1}{2}(A-A_n) + \left(\frac{1}{2}\right)^2 &\left\{\int\left(\sin\frac{A}{2}\right)^2\delta\frac{A}{2} - \int\left(\sin\frac{A_n}{2}\right)^2\delta\frac{A}{2}\right\} \\ +\left(\frac{1.3}{2.4}\right)^2 &\left\{\int\left(\sin\frac{A}{2}\right)^4\delta\frac{A}{2} - \int\left(\sin\frac{A_n}{2}\right)^4\delta\frac{A}{2}\right\} \\ &+\text{etc}\ .\end{aligned}$$

Or,

$$\frac{1}{2}\left(\mathrm{A}-\mathrm{A}_n\right)\frac{\mathrm{T}}{\tau\sqrt{\cos\phi}}=\int\left\{1+\left(\frac{1}{2}\right)^2\left(\sin\frac{\mathrm{A}}{2}\right)^2+\left(\frac{1.3}{2.4}\right)^2\left(\sin\frac{\mathrm{A}}{2}\right)^4+\text{etc}\right\}\delta\frac{\mathrm{A}}{2}$$

$$-\int\left\{1+\left(\frac{1}{2}\right)^2\left(\sin\frac{\mathrm{A}_n}{2}\right)^2+\left(\frac{1.3}{2.4}\right)^2\left(\sin\frac{\mathrm{A}_n}{2}\right)^4+\text{etc}\right\}\delta\frac{\mathrm{A}}{2}\;.$$

Whence, if we denote by $\frac{1}{2}\psi\mathrm{A}$ and $\frac{1}{2}\psi\mathrm{A}_n$ each of these integrals, we have

$$\left(\mathrm{A}-\mathrm{A}_n\right)\frac{\mathrm{T}}{\tau\sqrt{\cos\phi}}=\psi\,\mathrm{A}-\psi\,\mathrm{A}_n \qquad \text{And}$$

$$\tau=\mathrm{T}\sqrt{\sec\phi}\,\frac{\mathrm{A}-\mathrm{A}_n}{\psi\mathrm{A}-\psi\mathrm{A}_n}\;.$$

A table, then, of the values of $\psi\mathrm{A}$ would enable us at once to make the proper reductions.

Integrating by means of the preceding formula, we find

$$\begin{aligned}\psi\mathrm{A}=\mathrm{A}&\left\{1+\left(\frac{1}{2}\right)^3+\left(\frac{1.3}{2.4}\right)^3+\left(\frac{1.3.5}{2.4.6}\right)^3+\text{etc}\right\}\\ &-\sin\mathrm{A}\left\{\left(\frac{1}{2}\right)^3+\left(\frac{1.3}{2.4}\right)^3+\left(\frac{1.3.5}{2.4.6}\right)^3+\text{etc}\right\}\\ &-\frac{2}{3}\sin\mathrm{A}\left(\sin\frac{\mathrm{A}}{2}\right)^2\left\{\left(\frac{1.3}{2.4}\right)^3+\left(\frac{1.3.5}{2.4.6}\right)^3+\text{etc}\right\}\\ &-\left(\frac{2.4}{3.5}\right)\sin\mathrm{A}\left(\sin\frac{\mathrm{A}}{2}\right)^4\left\{\left(\frac{1.3.5}{2.4.6}\right)^3+\text{etc}\right\}\\ &+\text{etc}\;.\end{aligned}$$

a formula remarkable for its symmetry, but unfortunately of no use in practice, since the coefficients are all infinite.

But if the integration be performed by means of the formula

$$\int\sin Z^n\,\delta Z=\cos Z\left\{\frac{1}{n+1}\sin Z^{n+1}+\frac{1}{n+1}\,\frac{n+2}{n+3}\sin Z^{n+3}+\frac{1}{n+1}\,\frac{n+2}{n+3}\,\frac{n+4}{n+5}\sin Z^{n+5}+\text{etc}\right\}$$

the value becomes, after several reductions,

$$\begin{aligned}\psi\,\mathrm{A}=\mathrm{A}&+\sin\mathrm{A}\left(\sin\frac{\mathrm{A}}{2}\right)^2\cdot\frac{1}{3}\left(\frac{1}{2}\right)^2\left\{1\right\}\\ &+\sin\mathrm{A}\left(\sin\frac{\mathrm{A}}{2}\right)^4\frac{1}{5}\left(\frac{1.3}{2.4}\right)^2\left\{1+\left(\frac{4}{3}\right)^3\right\}\\ &+\sin\mathrm{A}\left(\sin\frac{\mathrm{A}}{2}\right)^6\cdot\frac{1}{7}\left(\frac{1.3.5}{2.4.6}\right)^2\left\{1+\left(\frac{6}{5}\right)^3+\left(\frac{6.4}{5.3}\right)^3\right\}\\ &+\sin\mathrm{A}\left(\sin\frac{\mathrm{A}}{2}\right)^8\cdot\frac{1}{9}\left(\frac{1.3.5.7}{2.4.6.8}\right)^2\left\{1+\left(\frac{8}{7}\right)^3+\left(\frac{8.6}{7.5}\right)^3+\left(\frac{8.6.4}{7.5.3}\right)^3\right\}\\ &+\text{etc}\;.\end{aligned}$$

Or,

$$\begin{aligned}\psi A = A &+ \frac{1}{2^2.3}\sin A\left(\sin\frac{A}{2}\right)^2 \\ &+ \frac{91}{(2.4)^2.3.5}\sin A\left(\sin\frac{A}{2}\right)^4 \\ &+ \frac{23031}{(2.4.6)^2.3.5.7}\sin A\left(\sin\frac{A}{2}\right)^6 \\ &+ \frac{12\,949\,497}{(2.4.6.8)^2.3.5.7.9}\sin A\left(\sin\frac{A}{2}\right)^8 \\ &+ \frac{13\,793\,405\,625}{(2.4.6.8.10)^2.3.5.7.9.11}\sin A\left(\sin\frac{A}{2}\right)^{10} + \text{etc}\end{aligned}$$

Here the order of the denominators is sufficiently obvious. That of the numerators can be exhibited thus: Let A, B, C, D, E, etc. represent the successive numerators, then

$A = 1$
$B = A.\ 4^3 + 3^3$
$C = B.\ 6^3 + (3.5)^3$
$D = C.\ 8^3 + (3.5.7)^3$
$E = D.\ 10^3 + (3.5.7.9)^3$, etc .

By means of which formulæ, logarithms of the coefficients have been computed.

At the first glance, however, it will be apparent that the value of ψA given in terms of the radius will be exceedingly inconvenient for computation, since it would be necessary to convert the arcs also into decimal parts of the same unit. It would be more convenient to take some small arc, as one minute, for the unit; in this case, all the coefficients would need to be multiplied by the constant $\frac{10800'}{\pi}$, or their logarithms to be increased by the logarithm of that fraction. The formula then becomes

$$\psi' A = A' + \frac{10800}{\pi}\,\frac{1}{2^2.3}\sin A\left(\sin\frac{A}{2}\right)^2 + \text{etc}\ ;$$

and its application is given by the equation

$$\tau = T\sqrt{\sec\phi\frac{A' - A'_n}{\psi' A - \psi' A_n}}\ ,$$

the accents denoting that the quantities are taken in minutes of a degree.

The computation of a table of the values of $\psi' A$ being very laborious, the operations have not yet been revised. In this case it is deemed advisable to postpone their publication until the revision is completed.

60. North Bridge,
22d May 1835.

Progressive Increase of Temperature as we descend in the Crust of the Earth.

For the purpose of ascertaining whether a constant stream of water could be obtained by means of an artesian well, sunk on the south side of the Jura mountains, at the distance of about a league from Geneva, and at an elevation of 297 feet above the level of the lake, M. Giroud, at his country residence at Pregny, bored to the depth of 547 feet without success. Despairing of success, he offered great facilities to any persons who might wish to prosecute the enterprize, for the purpose of scientific inquiry. On this occasion MM. Aug. De la Rive and F. Marcet made a successful application to the friends of science, and also to the government, and funds were obtained sufficient to enable them to continue the operations during eight months, and to extend the boring to the depth of 682 feet. The hole bored was about four and a half inches in diameter. Water began to appear in it at the depth of twenty feet, and it is worthy of remark, that the height at which the water stood in the opening, as measured from the surface, was lower when the greatest depth was obtained, than it was at half the depth. At 275 feet of depth, the water stood at 14 feet from the surface; at 500 feet it sunk to 22 feet; at 550 feet, to 35 feet. It then rose. At 595 feet, it stood at 24 feet 6 inches, but at 675 feet, it again sunk to 35 feet 8 inches. The result of this praiseworthy effort must operate as a salutary preventive from any farther expensive attempts to obtain running fountains from the theory of an internal communication with the springs on the summits of the Jura.

Having attained the extraordinary depth above mentioned, the experimenter devised the means of ascertaining the temperature of this opening at different depths. As the common thermometer would not answer the purpose, they contrived a self-registering thermometer, constructed on a large scale, and whose accuracy was subject to the most satisfactory tests.

The following Table exhibits the temperature of the bore-hole at the depths specified.

Depth below the Surface in Feet.	Corresponding Temperature.
30	8.4 Reau.
60	8.5
100	8.8
150	9.2
200	9.5
250	10.0
300	10.5
350	10.9
400	11.37 Reau.
450	11.73
500	12.20
500	12.63
600	13.05
650	13.50
680	13.80

It thus appears that the increase of temperature below the depth of 100 feet from the surface, as far down as 680 feet, is precisely 0°.875 of Reaumur (=1°.968 or 2° Fahrenheit, very nearly), for every 100 feet. It will be observed, that the increase, instead of moving *per saltum*, as in some other cases, moves with remarkable uniformity. This, the experimenters think, may be owing to the care which was taken in this case to remove and avoid every source of error.

This experiment appears to be the first attempt to ascertain, with any accuracy, the temperature of the earth at considerable depths, among the mountains of Switzerland. The geological structure of the beds which were bored through on this occasion, was as follows: next to the upper layer of vegetable earth, sand and gravel, was a gravelly and bluish clay, mingled with soft sandstone (molasse). Below 120 feet commenced a succession of beds of marl and soft sandstone, of various thicknesses, which continued without interruption to the termination of the boring, 682 feet. At 220 feet there was a bed of coarse sandstone (molasse grossiere) two feet thick, with rolled pebbles, a remarkable fact, considering the depth. A strong fetid sulphurous odour was also observed in the layer of yellow marl mixed with sandstone, at the depth of 280 feet, that is near the level of the lake, and a grain of salt was found in the sandstone at this depth. The sulphurous odour again appeared at 600 feet, without the presence of any sulphurous compound that would account for its origin.

Researches on Wines and other Fermented Liquors. By LEWIS C. BECK, M. D., Professor of Chemistry and Botany in the University of the City of New York, &c. &c.

HAVING recently been engaged in a series of experiments to determine the proportion of alcohol contained in several kinds of wine, and other fermented liquors, I was induced to examine some other points connected with their history; and now present for publication, in a condensed form, the results of my inquiries.

Composition of Wine.—The composition of wine is very variable. The substances found in it are water, alcohol, undecomposed sugar, gum, extractive matter, vegetable albumen, acetic acid, bitartrate of potassa, tartrate of lime, tartrate of alumina and potassa, sulphate of potassa, chloride of sodium, and, in the red wines, red colouring matter, and, in those of champaigne, carbonic acid.

Acetic and Carbonic Acids.—Acetic acid is often found in the wines from northern countries, and in altered wines, it is formed at the expense of the alcohol. Almost all wines, however, exhibit the acid reaction. In champaigne, it is owing to free carbonic acid, but in others it is due to the bitartrate of potassa. The effervescence which is observed on adding carbonate of potassa to wine, and the subsequent precipitation, are the results of the action of this excess of tartaric acid, and the consequent liberation of a portion of tartrate of lime, which this salt most generally contains.

Malic Acid.—It has been frequently stated, that wine contains malic acid, and a malate of lime is said, by Chaptal, to be formed by the addition of lime water to wine;—an opinion which seems also to be countenanced by Mr Brande. But the existence of this acid is rendered doubtful, if not disproved, by the fact that an insoluble precipitate results, as well from the addition of ammonia or potassa, as of lime. Now, the malates of these alkalies are very soluble, and hence we may more safely ascribe the precipitate thrown down in all these cases to the saturation of the excess of tartaric acid contained in the bitartrate of potassa, by which means the insoluble tartrate of lime, which is usually combined with it, is liberated.

Vegetable Albumen.—Grapes, according to Berzelius, contain a small portion of this substance, which he has described in the fifth volume of his elaborate treatise on chemistry. In examining a specimen of American wine, which was said to be the pure juice of the grape, I found, that when evaporated to about one-eighth of its bulk, upon adding a portion of alcohol, there was a deposit of a strong dark coloured matter, soluble in water, and in solution of ammonia, but insoluble in sulphuric acid. In these respects, it agrees very well with the description of vegetable albumen given by Berzelius. A portion of pure Madeira, when treated in the same manner, yielded a bulky white precipitate of saline matters.

Sulphate of Potassa.—The presence of sulphuric acid in wines is distinctly shewn by the dense precipitate which results from the addition of muriate of barytes. I am not aware that any other ingredient would produce this effect, except carbonic acid, but the carbonate would be soluble in muriatic acid, which is not the case with the precipitate in question.

Tartrates of Potassa and of Lime.—The bitartrate of potassa is one of the most abundant of the solid ingredients of wine, and the tartrate of lime, as has already been remarked, is generally associated with it. It is probably owing, in a great measure, to the presence of these salts, that such dense precipitates are produced upon adding to wine the acetate of lead, or the nitrates of tin, mercury, or silver. Insoluble tartrates of the metallic oxides are thus formed.

Tartrate of Alumina and Potassa.—This salt, according to Berzelius, is especially characteristic of the German wines.

Colouring Matter.—In the light-coloured wines, the colour is supposed to be derived from the extractive matter; but in the red wines there exist tannin and colouring matter, the last of which may be obtained, according to Robiquet, in a crystalline form.

Red wines are sometimes imitated by the dealers in wine, by adding to white wine other colouring matter, as, for example, Brazil wood, logwood, the red beet, elder berries, &c. The detection of these falsifications has engaged the attention of many chemists.

Vogel proposes the mixing of the suspected wine with the subacetate of lead. Pure wine gives, with this re-agent, a greyish-green precipitate; wine that has been coloured by Brazil wood or elder berries gives a precipitate of an indigo-blue colour; and when the red beet or sandal wood has been employed, the precipitate is red. The following are the results of my experiments with the subacetate of lead:

When added to pure Madeira wine, the precipitate was of a light yellow (cream) colour.

Pure Port wine gave it a greyish precipitate, with a slight tint of green.

With infusion of logwood, the precipitate produced by the subacetate was of a deep purple; when the colouring matter was largely diluted with water, the precipitate was less dense, and of a lead colour.

With infusion of the red beet, the precipitate was of puce colour; when largely diluted, the precipitate was of a pale red (salmon) colour. In one of the wines which I examined, the subacetate of lead threw down a bulky purple precipitate, similar to that produced by its addition to infusion of logwood. I received this wine under the name of Torres Vedras. It was of a very dark colour, and was represented to be an old wine.

Berzelius states, that the colouring matter of red wines gives different coloured precipitates with subacetate of lead, according to the age of the wine. Thus, in new red wine, the subacetate commonly throws down a blue precipitate. This circumstance must greatly impair the value of this test.

Nees d'Esenbeck has proposed a method of detecting artificial colouring matter in wine, which is said to be more certain in its indications. This consists in dissolving one part of alum in eleven parts of water, and one part of carbonate of potassa in eight parts of water. The wine is mixed with its own bulk of the solution of alum, which renders its colour more bright. To this, the alkaline solution is now added, little by little, taking care not to precipitate the whole of the alumina. The alumina precipitates with the colouring matter of the wine in the form of a lake, whose shade of colour varies with the nature of the colouring matter, and which, when combined with an excess of potash, assumes another tint, also varying with the colouring matter combined with the alumina. In order to obtain correct results, it is necessary to make comparative experiments with pure wine.—See *Berzelius's Traité de Chimie.*

Specific Gravity of Wine.—It has long been known, that the specific gravity of wine gives us no information, as it does in the case of distilled liquors, of the proportion of alcohol which it contains. Direct experiments on this point have been made by Brisson and Brande. I also accurately determined the density of several varieties of wine, and other fermented liquors. The following are some of the results :—

Madeira, mean of three kinds, . . .	0.98659
Secrial Madeira, ...	0.98606
London Particular, ...	0,98860
Port, mean of two kinds, ...	0.98203
Sauterne, ...	0.99511
Claret, mean of two kinds, ...	0.99490
American Wine, ...	1.00702
Cider, mean of two kinds, ...	1.03400
Metheglin, ...	1.08964

Alcohol in Wine.—It has been a subject of some controversy whether alcohol exists, *ready formed*, in wine, or whether it is generated by the heat employed in the process of distillation. The latter opinion was supported by Fabroni (Ann. de Chim. xxx. 220.) ; but its fallacy has been completely exhibited, by the able investigations of Mr Brande and Gay Lussac.

The following statements seem to be conclusive as to the existence of *ready formed* alcohol in wine.

1. Alcohol can be obtained from wine by distillation, in *vacuo,* at the temperature of 60° F., which precludes the idea that it is formed by the action of heat upon the elements existing in the fermented liquor.

2. When a portion of wine is partly distilled off, and the distilled liquor is afterwards added to the residuum in the retort, the specific gravity of the mixture is precisely the same as that of the wine previous to distillation. Alcohol being much lighter than wine, if it were formed during the process of distillation, would have the effect of reducing the specific gravity, when added to the residuum, which is never the case.

3. When the colouring and extractive matters in the wine are precipitated by the subacetate of lead, the pure alcohol may be

separated by the subsequent addition of dry subcarbonate of potassa, in the same manner as from whisky, gin, and brandy.

The first of the above statements has been shewn to be true by Gay Lussac (Ann. de Chim. lxxxvi. 175). The correctness of the second was demonstrated by Mr Brande, upon a suggestion, contained in a notice, of his first paper contained in the Edinburgh Review. I confirmed the results of Mr Brande's experiments on this point, in the case of three kinds of wine, viz. Madeira, Torres Vedras, and Claret.

The last, however, is the most conclusive of all proofs in favour of this view of the constitution of wine, as by the process here referred to, the alcohol may be separated from wine without the intervention of heat. We are indebted to Mr Brande for having first pointed out a mode of effecting this object (Philosophical Transactions for 1813). This consists in adding to the wine a solution of subacetate of lead, filtering the liquor, and then adding to the filtered liquor dry subcarbonate of potassa. The metallic oxide, as he says, forms a dense precipitate with the acid and colouring extractive matter of the wine; by filtration a colourless fluid is obtained, from which the alcohol may be separated, as above mentioned. Mr Brande also states, that the acetate of lead and subnitrate of tin, produce the desired effect of separating the colouring and acid matters, in the greater number of instances; and to these, I may add, the protomuriate of tin, and the protonitrate of mercury, which I found to answer in most cases.

Observing the effect of adding the subcarbonate of potassa to wine, viz. that of causing effervescence, and the forming of a flocculent precipitate, I was led to infer, that the compound thus formed interfered with the separation of the alcohol. To determine whether this opinion was correct, I added the subcarbonate to a portion of wine, as long as it produced the effect just mentioned, when the whole was thrown upon a filter. The filtered liquor was of a somewhat darker colour than the wine; but when the subcarbonate was now added to it, the separation of the alcohol was speedily effected.

This result was also produced, when I employed a solution of ammonia, instead of the carbonate of potash; the flocculent precipitate thus formed, being separated by filtration, the alcohol appeared, as in the former case, upon the addition of a due proportion of subcarbonate of potassa. These experiments seem to prove, that the separation of alcohol in wine, by the common mode of adding subcarbonate of potassa, is prevented by the tartrate of lime, which is liberated by the first addition of an alkali, and which, perhaps, together with some of the other matters, forms a flocculent mass suspended in the liquor. In my opinion, they constitute a more decisive proof of the existence of ready formed alcohol in wine, than any which has yet been offered.

In the analysis of wines and other fermented liquors, the results of which are given below, the following process was adopted, for the purpose of determining the proportion of alcohol which they contain.

A glass bottle, with a long and narrow neck, capable of holding 1020 grains of distilled water, was filled with the wine under examination. This quantity of wine was now put into a glass retort, which was carefully luted to a receiver, so as to prevent the escape of vapour. A gentle heat was applied to the retort, while the receiver was kept constantly cold, by the dropping of water upon it. The heat being cautiously managed, towards the end of the process I was enabled to distill off nearly the whole of the wine, without burning the solid residuum. Thus, from 1008 grains of Madeira, I distilled over 979 grains, while from the Port there was a loss of nearly 50 grains, in the same measure, owing chiefly to the greater proportion of residuary matter. To make up for this loss, I added wine, of the same kind as that distilled, so as to bring it to the original measure of the wine, excepting a small allowance for the space occupied by the solid ingredients. These were now shaken together, and allowed to remain for some time, and the specific gravity then carefully determined at the temperature of 60° Fahr. From this the proportions of alcohol and water *by weight*, were ascertained by a reference to the Tables of Mr Gilpin, published in the Philosophical Transactions for 1794, and, by calculation, the proportions *by measure* were estimated.

This process, it will be observed, is essentially that of Mr Brande, and that my results may be easily compared with his, I have referred them to the same standard which he adopted. I may also state, that I compared these results in one or two cases with those produced by the process of precipitation, by subacetate of lead, and subsequent separation of the alcohol by subcarbonate of potassa. They were found to be so nearly coincident, that I did not think it worth while to extend this mode of investigation.

Table shewing the Proportion of Alcohol per cent. by Measure, contained in several kinds of Wine and other Liquors; the Specific Gravity of the standard Alcohol being 0.825, *at the temperature of* 60° *F.*

Kinds of Liquor.	Proportion of Alcohol per cent. by measure.
1. Madeira, common,	25.77
2. Ditto imported from the house of Robert Seal,	23.11
3. Ditto common,	22.41
4. Ditto imported from the house of Houghton and Co.,	22.25
5. Ditto "Farquhar" in bottle, forty years old,	21.79
6. Ditto twenty years old,	21.45
7. Ditto "Edgar,"	21.30
8. Ditto "Brammin,"	20.91
9. Ditto common,	20.72
10. Ditto "Wanderer,"	20.70
11. Ditto "Blackburn," old,	20.68
12. Ditto said to be the pure juice of the grape, twenty-eight years old,	19.30
13. Sercial Madeira,	25.18
14. Ditto ditto,	18.96
Average of fourteen kinds,	21.75
15. London Particular,	22.10
16. Bucellas,	18.80
17. Brown Sherry,	18.03
18. Port, seven years in bottle,	22.87
19. Port,	22.35
20. Ditto,	21.98
Average of 18, 19, and 20,	22.60
21. Torres Vedras,	20.51
22. Sauterne,	13.00
23. Claret, Chateau Margeaux,	11.80
24. Ditto Palmer Margeaux,	11.04
Average of 23 and 24,	11.42
25. American Wine, two years old,	11.25
26. Metheglin, twenty years in bottle,	10.57
27. Ale, Albany, in bottle two years,	10.67
28. Ale, Albany, in barrel,	7.38
29. Cider in bottle,	4.80
30. Ditto in barrel six months,	4.84
31. Ditto in barrel,	4.41
Average of 29, 30, and 31,	4.68
32. Irish Whisky, imported in 1825,	73.70
33. Gin, genuine Hollands,	55.44
34. Brandy, common,	51.01
35. Whisky, common,	42.95
36. Spirits of Wine, obtained at the druggists,	93.27
37. Spirits of Wine,	95.35

It is proper to state, that this investigation was commenced at the request of E. C. Delavan, Esq., whose philanthropic exertions in the cause of temperance are so well known, and so justly appreciated. From him, and from other gentlemen in the city of Albany, I received, with a few exceptions, all the specimens of wines and other liquors which I examined. At my suggestion, each specimen was accompanied with a label, containing a notice of its reputed age and character, of which I have introduced only so much in this Table as was necessary to identify and distinguish it. I should observe, that most of the wines are said to be among the purest and best which are brought to this country.

The results in the above table agree generally with those of Mr Brande. In all cases where the difference was marked, as in Nos. 13, 14, 22, 23, and 24, the trials were repeated several times, and the mean of these is given. The ale, No. 27, contains more alcohol than any put down in the Table of Mr Brande, as ordinarily published; but in the Journal of Science and the Arts (vol. v. p. 124), he states, that Lincolnshire ale, brewed by Sir Joseph Banks, contained 10.84 per cent. of alcohol. Our cider, it would seem, contains less alcohol than the lowest average of the specimens examined by Mr Brande, which is 5.21 per cent.

Addition of Brandy to Wine.—An opinion has been recently advanced, that the large proportion of alcohol which some wines contain, is due to the addition of brandy to the *must;* and it has even been maintained, that, without such addition, wines speedily undergo the acetous fermentation, and thus lose their peculiar flavours. These opinions, if correct, must render quite fallacious the results of the analyses of the older wines, and they deserve, therefore, to be carefully examined.

In regard to this point, I avail myself, in part, of the information contained in the excellent treatise on Domestic Economy by Mr Donovan.—(*Lardner's Cabinet Cyclopædia.*)

Brandy is not added to wines in France or Germany; the finer wines, claret, burgundy, and hock, are said to be totally destroyed by it. But the practice is quite common, nay, almost universal, in the wines of Spain, Portugal, and Sicily, which are intended for foreign markets. The reason of this I apprehend is, not that the wines cannot be kept without such an admixture, but that these strong wines are in great repute, and perhaps, also, that, with the addition of brandy, less care is required in preparing them for exportation.

That wines may be kept for a great number of years without the admixture of brandy, is evident, from the age of many ancient wines. Horace speaks of wine that is nearly seventy years old; and the Opimian wine, which had been made in the time of the Consul Opimius, was two hundred years old. In order to preserve their wines to these ages, the Romans concentrated the *must* or grape juice of which they were made, by evaporation, either spontaneously, in the air, or over a fire, and, so much so, as to render them thick and syrupy.

This process of evaporation, however, was by no means necessary to their being preserved; for wines, not treated in this manner, have been known to keep equally long. We are informed by Neuman, "that the tartish German wines keep the longest of any; some of them have kept 200 or 300 years; and in Strasburgh, there is a cask 400 years old, and many of them above seventy; the wine being occasionally racked off into smaller casks, that the vessel may be continually full. These very old wines are preserved, rather for curiosity than use, as they not only grow too strong for drinking, but at last quite disagreeable."

The preservation of wines for so long a time, when the process of distillation was still unknown, and in cases where no brandy had been added, as in the German wines referred to by Neuman, seems to prove conclusively, that the admixture of brandy, or other distilled liquor, is not necessary to effect this object. Nor is it probable that the strength of the wine is much influenced by the brandy, as ordinarily employed. The pure juice of the grape, after a few years, becomes fully as alcoholic as those wines which have been brandied. Mr Brande procured port wine, sent from Portugal, for the express purpose of ascertaining how long it would remain sound, without any addition whatever of spirit having been made to it, but it did not differ materially, in the proportion of alcohol, from other kinds. Moreover, the raisin wine, which had been fermented without any addition of spirit, contained a larger amount of alcohol than any other wine in his tables.

As the alcohol in natural wines is the produce of the sugar contained in the grape, if any part of the sugar escapes decomposition, the wine will contain alcohol and unaltered sugar, and will be *sweet*. Now, in those grapes which contain a large proportion of sugar, and in which there is a sufficiency of yeast present to decompose it, there will be a superabundance of alcohol. But the alcohol thus formed stops the fermentation, and the same effect is also produced by the admixture of brandy or spirit.

On the contrary, where the relative quantities of yeast, sugar, and water are such as will conduce to a perfect attenuation, the fermentation will proceed until the whole of the sugar is converted into alcohol. The result under such circumstances will be a full-bodied, spiritous, sound, and, as it is technically called, a dry wine. The addition of alcohol, during the fermentation of the *must*, therefore, is to be conducted upon fixed principles, and with a strict reference to the deficiencies in the ingredients of the grape. An indiscriminate admixture of spirit, either during the fermentation, or after that process has ceased, would be attended with hazard to the flavour and value of the wine.

It follows, from these remarks, that alcohol is generated during the process of fermentation, and that its amount depends upon the proportion of saccharine matter in the grapes, and that when all the ingredients are in due proportion, the most sound and spiritous wines are obtained; that, when this is the case, the wine may be preserved for any length of time, without the addition of spirit in any form; and that, when this addition is made, it is only for the purpose of supplying deficiencies in the *must*, or, in other words, to bring the wine to that degree of strength which it would naturally have attained if all the ingredients of the *must* had been in such proportion as to effect a perfect attenuation.

Wines of Palestine.—In the discussions which have recently taken place concerning the chemical nature and effects of wines, some opinions have been advanced concerning the wines of Palestine, which deserve a little consideration. It has been supposed, that the wine spoken of in various parts of sacred history was far less spiritous than that of modern times; and some have even gone so far as to assert, that *all* modern wines are *brandied*, and that to this circumstance is to be ascribed the large proportion of alcohol which they are found to contain. Upon consulting the original papers of Mr Brande, however, it will be found, that that acute chemist was not ignorant of the fact, that many wines are artificially brandied; and as the very object of his researches was to prove the existence of ready formed alcohol in natural wines, he would, of course, be careful to select those which were free from admixture. Indeed, he expressly states that he used this necessary precaution; and, moreover, Gay Lussac, though in the very country where many of the wines analyzed by Mr Brande were produced, confirms and quotes his results, without expressing the least doubt of their accuracy from this cause. It is therefore probable, that in most of the wines which were examined by Mr Brande and by myself, the whole amount of alcohol was due to the fermentation of the *must.* The differences in this amount depended upon the kind of grape, and upon the influence of climate, soil, and culture. These facts being assumed, we shall have some guide in our subsequent inquiries.

The wines of Palestine are generally represented by modern travellers as being of excellent quality. The sweet wines are particularly esteemed in the East, because they are grateful to the taste, very exhilarating, and will keep, some of them, for a long time. They were therefore preferred by those addicted to drinking, and commonly selected for the tables of kings. (*Paxton's Illustrations.*) The prophet Joel, accordingly, describes a state of great prosperity by the figure of mountains dropping down new, or, more correctly, *sweet* wine, (c. iii. v. 18.) Their inebriating quality is alluded to by the prophet Isaiah: " I will feed them that oppress thee, with their own flesh; and they shall be drunken with their own blood, as with sweet wine," (c. xlix. v. 26.) And the privation of this enjoyment is placed by the prophet Micah among the judgments which the Almighty threatened to bring upon his ancient people for their iniquity: " Thou shalt tread the vintage of sweet wine, but shalt not drink wine," (c. vi. v. 16.)

Thus the testimony of travellers, concerning the spiritous nature of the wines of Palestine, accords with that of the sacred writers. The ancient wines are said to have been mixed with water for common use; but it is evident that this practice did not prevail among the Jews, for Isaiah, in mentioning a mixture of wine and water, evidently means to express, by the phrase, the degenerate state of his nation. " Thy silver is become dross, thy wine mixed with water," (c. i. v. 22.) It is observed, by Thevenot, that the people of the Levant never mingle water with their wine at meals, but drink by itself what water they think proper for abating its strength. While the Greeks and Romans, by mixed wine, understood wine united and lowered with water, the Hebrews, on the contrary, meant by it wine made stronger and more inebriating, by the addition of powerful ingredients, as honey, spices, &c., or wine inspissated, by boiling it down to two-thirds, or one-half of the quantity, myrrh, opiates, and other strong drugs being added. (*Paxton's Illustrations.*) And severe denunciations against the use of this drink are contained in various parts of the sacred Scriptures.

Moreover, the grapes of Palestine were remarkable for their size and richness. The account given by Moses of the bunch of grapes, brought by the spies to the Israelitish camp (Num. xiii. 24), is confirmed by the statements of several travellers. Doubdan assures us that in the valley of Eshcol were bunches of grapes of ten or twelve pounds. Forster tells us, that he was informed by a Religious who had lived many years in Palestine, that there were bunches of grapes in the valley of Hebron so large that two men could scarcely carry one, *i. e.* without bruising or blemishing the berries. Indeed, travellers generally concur in their high commendation of the grapes of that country.

To these facts I will only add, that the wines of Palestine were generally kept in bottles made of leather or goat skins, firmly sewed or pitched together. In these, the process of fermentation took place, and the wine acquired its proper degree of strength.

In the absence of any thing like chemical analysis, these are the data from which we must draw our conclusions concerning the nature of the wines referred to by the sacred writers. Some of them are represented to have been sweet wines, which, if not the strongest, are known to be among the stronger kinds. The grapes from which they were produced, were remarkable for their richness and excellence, the climate of the country being such as to favour their growth, and the development of those principles which, during fermentation, are converted into alcohol; and, as the grapes of that country are known to furnish very rich and spiritous wines, we may infer, that the ancient wines were similar in their character, since there is abundant evidence that the climate has not suffered any material change for 3000 years.

I should not omit, in confirmation of this view of the spiritous nature of the wines of Palestine, to advert to the modes in which they were kept. It is now well known, that, when mixtures of alcohol and water are put into bladders, the water evaporates, and leaves the alcohol in a more concentrated form; and it is asserted, that wine which has been kept in bottles closed by pieces of bladder firmly tied over the mouth, in a few weeks acquires the strength and flavour which would be imparted to it only by several years preservation in the ordinary way. Now, it is probable that the leathern bags into which these wines were put, would produce a similar effect upon the liquor, which, after the process of fermentation had ceased, would soon attain its complete and appropriate alcoholic character.

Intoxicating Power of Wine.—It is generally supposed that, in wine, the action of the alcohol upon the animal economy is modified by the other vegetable matters which are mixed or combined with it. According to this view, it is of course taken for granted, that the intoxicating power of wine is not so great as that of a mere mixture of the same proportion of alcohol with water. Before offering any remarks upon this point, it may be proper to introduce the following table, shewing the relative powers of several wines and other fermented liquors, on the supposition that the alcohol is equally effective as in distilled liquors:—brandy containing 53.39 per cent. of alcohol being taken as the standard, and set down as 100.

Brandy,	100.	Sauterne (22.),	24.34
Strongest Madeira (1.),	48.26	Claret (average),	21.38
Weakest Madeira (6.),	36.14	American Wine (25.),	21.07
Port (average),	42.33	Metheglin (26.),	19.79
Bucellas (16.),	35.21	Ale (27.),	19.98
Sherry (17.),	33.75	Ale (28.),	13.82
Torres Vedras (21.),	38.22	Cider (average)	8.76

From this table it appears, that two measures of strong Madeira are equivalent, in the amount of alcohol which they contain, to nearly one measure of brandy, and that about five measures of ale are equivalent to about one of brandy. It will perhaps be quite generally asserted, that the intoxicating powers of these liquors are not in the proportions thus expressed, and hence the opinion, that the effect of alcohol in wines and other fermented liquors is modified by the other vegetable matters which they contain. I apprehend, however, that the difference is not so great, all things being equal, as might at first be supposed. The following facts appear to me to throw some light on this subject.

New wine is said to be more intoxicating than that which is old, although the latter is usually more spiritous. The reason of this undoubtedly is, that the alcohol by time becomes more intimately combined with the water, and thus to a certain extent loses its power of intoxication. The union of alcohol and water is not complete until they have been for some time in contact; and hence, when brandy and water are taken into the stomach immediately after their mixture, the effect on the system is not very different from that produced by the same proportion of brandy taken separately.

Mr Brande, in one of his papers, assures us that, when brandy and water are mixed and allowed to remain in combination for some time, the intoxicating power is not greater than that of wine containing an equivalent of brandy. In wines, the union of the alcohol and water becomes complete by the process of attenuation, and it is, in my opinion, to this more than to the controlling effects of the other vegetable matters that we are to ascribe their less decided intoxicating powers; and, on the contrary, it is to the imperfect union that the ordinary mixtures of brandy and water owe their more energetic action on the system. I should also observe, that mistakes concerning the relative intoxicating powers of mixtures of alcohol and water, and of wines, may have arisen from the different modes in which they are ordinarily drank. A half pint glass of brandy and water, of common strength, contains an amount of alcohol but little less than the same measure of ordinary Madeira. And, if these portions of wine and of brandy and water should be drunk in the same manner, the effects on the animal economy would not be so different as is generally supposed. Wine is usually taken in small quantities and at intervals,—circumstances which must have a great effect in modifying its action on the system, and to these may also be added the fact, that its habitual use impairs the susceptibility of the system to its intoxicating power.

On the whole, there is reason to conclude that the difference in the intoxicating power of wine and that of the ordinary mixtures of water with the same proportion of alcohol, if it exists at all, is owing more to the intimate combination of the alcohol with the water in the former, than to any peculiar effect of the other vegetable matters contained in it. But, from the considerations above stated, I am inclined to believe, that, after all, the difference is rather apparent than real.—*Amer. Jour. Science and Arts*, vol. xxviii. p. 42.

On the Action of Voltaic Electricity on Alcohol, Ether, and Aqueous Solutions.[*] By ARTHUR CONNELL, Esq. F.R.S.E. Communicated by the Author.

I WAS led into the following investigations, from observing that when alcohol, holding in solution a minute quantity of pure potash, as $\frac{1}{300}$th part, was acted on by a small battery of fifty pairs of two-inch plates, the poles employed being of platinum foil, evident signs of decomposition were exhibited by the evolution of elastic fluid from the negative pole, whilst none appeared at the positive. This experiment recalled to my recollection a statement made a few years ago by Dr Ritchie,[†] that when alcohol, not holding any substance in solution, was acted on by a powerful battery, gas was given off at the negative pole, which Dr Ritchie stated to be olefiant gas; and the conclusion which he drew was, that the alcohol had been resolved into water and olefiant gas. I was therefore naturally led to conjecture, that the gas liberated in my experiment might be olefiant gas; but on examining it, both by means of chlorine and in the usual way in the voltaic eudiometer, it proved to be hydrogen, mixed, when collected from alcohol, in contact with atmospheric air, with a variable proportion of the constituents of atmospheric air, which had been held in solution by the liquid, but quite pure when the alcohol was previously exposed to the vacuum of an air-pump, and then acted on in a close tube. By placing the foils side by side, as was done in this latter experiment, the quantity of gas liberated was considerably increased.

The same result was obtained whether alcohol, of sp. gr. .830, or of .7928 at 66° F., was employed. When $1\frac{1}{3}$ dram of the latter alcohol, holding $\frac{1}{200}$th of potash in solution, was acted on in a tube by 72 pairs of four inch plates, the platinum foil poles being placed side by side, at a short distance from one another, a cubic inch of hydrogen was obtained from the negative pole in less than $\frac{1}{4}$th of an hour, and $2\frac{1}{2}$ cubic inches in $2\frac{1}{4}$ hours from the commencement. During the action the liquid boiled, and became of a red colour from the formation of resinous matter, and a precipitation of carbonate of potash took place.

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[*] The above is an abstract of a paper read to the Royal Society of Edinburgh, 27th April 1835, but not yet published, with some additional matter.

[†] Phil. Trans. 1832, p. 285.

Small quantities of other substances soluble in alcohol, as chloride of calcium, iodide of potassium, nitrate of lime, boracic acid, &c., produced similar effects as potash, in causing an evolution of gas, but to a much less extent.

It was then found, that if alcohol sp. gr. .7928, at 66° Fahr. holding nothing in solution, was acted on by 216 pairs of four-inch plates, in a tube, the platinum foil poles being placed side by side within $\frac{1}{20}$th to $\frac{1}{30}$th of an inch of one another, gas was evolved as before, from the negative foil, and none from the positive; and this gas, which was collected sometimes over water, and sometimes over mercury, proved, on examination in repeated trials, to be hydrogen mixed with a small quantity of common air derived from the liquid. The alcohol, after the action, was found to contain a minute trace of resinous matter.

The effect of the presence of small quantities of the foreign substances, is to increase the conducting power of the liquid, as is shewn by an increased action on the galvanometer. The electric current is thus enabled to exercise its decomposing agency with greater facility.

The action, in these cases, is conceived to consist in the voltaic decomposition of water contained in the alcohol, apparently as a constituent, when absolute alcohol is acted on; the hydrogen being evolved from the negative pole, and the oxygen being absorbed by the fluid, and employed in producing certain secondary effects. We are prepared for adopting this view, by calling to mind the strong affinity which alcohol or its constituents have for oxygen, and the variety of circumstances under which an oxidation of alcohol ensues, as well as the variation, on the nature of the products, according to the energy of the oxidating circumstances. In the acetous fermentation, oxygen is absorbed, and acetic acid and water are formed. When alcohol is exposed to the nascent oxygen from oxide of manganese and sulphuric acid, or when an alcoholic solution of potash absorbs oxygen from the atmosphere, or when alcohol and ether are oxidated by the agency of glowing platinum,—in all these cases a more highly oxygenated acid, the formic, is produced in addition to the acetic.* The absorption of oxygen during the action of voltaic electricity on alcohol, is an analogous effect to these instances. Where the alcohol held $\frac{1}{200}$ of potash in solution, it was stated that carbonate of potash was precipitated, and that the liquid became red from the formation of resinous matter. The carbonic acid here comes in place of the formic and acetic acids, which are formed in a strong alcoholic solution of potash, exposed to the atmosphere, which is also known to acquire a red colour from the production of resinous matter. The formation of carbonic acid marks a more powerful oxidation; and accordingly, is only observed during voltaic action, when the disposing affinity of potash comes into play, and the electric agency is of an energetic description.

The positive gas could, in certain circumstances, be made to appear in addition to the negative, as by diluting the alcohol with an equal bulk of water, or by dissolving $\frac{1}{100}$ of potash in alcohol of sp. gr. .840; and also by arrangements producing certain electrical effects, as by reversing the battery after it had been some time in action, or by performing the experiment in metallic vessels, instead of those of glass or porcelain.

The view, that the hydrogen evolved from alcohol under voltaic agency proceeds from water contained in the alcohol, was farther confirmed, by passing the same electric current through alcohol sp. gr. .796 at 60° F., holding a minute quantity of potash dissolved, and through water holding the same quantity of potash dissolved, in the arrangement to which Dr Faraday has given the name of the volta-electrometer; when it was found that the same quantity of gas was evolved from both negative poles, shewing that in both solutions, water was the subject of decomposition.

In the whole circumstances, I conceive it to be quite clear that the hydrogen proceeds from decomposed water; but a question of some interest remains, Does the water thus decomposed enter into the constitution of the alcohol, when alcohol of very low sp. gr. is acted on? Although it has been very generally held that alcohol is a hydrate, this view has been hitherto rather a more or less probable theoretical inference from experimental results, than an experimental result itself; but if it can be shewn that alcohol still yields hydrogen from the negative pole, when it is of such specific gravity as to entitle it to the character of absolute alcohol, we shall then, I conceive, have direct experimental proof that water enters as such into the constitution of alcohol. The alcohol principally used in the preceding experiments had a sp. gr. of .7928 at 66° F., which corresponds with .795 at 60° F., and with .792 at 68° F., or 20° cent. It was obtained by Mr Graham's process, of exposing commercial alcohol in the vacuum of an air-pump with quicklime, during from four to five weeks, and renewing the lime after the first week's exposure. By continuing the exposure for eight weeks, I afterwards succeeded in obtaining alcohol of specific gravity .7928 at $62^\circ\frac{1}{3}$ F., corresponding to .7938 at 60° F., and to .790 at 20° cent. This alcohol,* when acted on in the tube, fig. 1, by 216 pairs of 4-inch plates, the foils being $\frac{1}{30}$ to $\frac{1}{40}$ of an inch apart, still yielded gas from the negative pole, although in smaller quantity than the alcohol of .792 at 20° cent. The diminution in quantity was, however, merely owing to the conducting power of the former alcohol being inferior to that of the latter; for when only $\frac{1}{1000}$ part of potash was dissolved in the alcohol of .790, it yielded gas in considerable abundance, comparatively speaking, and that with only 72 pairs of 4-inch plates; half a cubic inch being collected in ten minutes, and the evolution still going on. This gas, on examination, proved to be hydrogen, as in the former instances. Even when only $\frac{1}{10.000}$ part of the alkali was dissolved, the liquid still gave a considerable evolution of gas, and the dilution might have been carried much farther without destroying the effect. No reasonable doubt, therefore, I think, can be entertained, that water enters as such into the constitution of absolute alcohol.

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* New Edin. Phil. Journ. xxviii. 231.

Pure ether, rectified over chloride of calcium, was exposed to the action of 216 pairs of 4-inch plates, without the slightest symptom of decomposition, or action on a galvanometer, consisting of a single magnetic needle, 7 inches long, in the centre of 30 circuits of insulated copper-wire. Neither was it decomposed when it held corrosive sublimate, chloride of platinum, or chromic acid in solution. I therefore consider the conclusion authorized, that ether does not contain water as a constituent.

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* The experiments with this alcohol have been made since the paper was read to the Royal Society of Edinburgh.

Assuming it to be proved that absolute alcohol does, and that ether does not, contain water as a constituent, the most probable view appears to be that alcohol is, as Liebig supposes, a hydrate of ether; but the non-action of the pile on the latter fluid appears to me to be unfavourable to Liebig's view, that ether is the oxide of an unknown radicle, because on that view, it would seem that it ought to be resolved by voltaic agency into its electro-negative and electro-positive constituents.

In the course of these researches I was led to examine experimentally the evidence for Dr Faraday's principle of the definite action of the electric current, in so far as regarded solutions, and found it fully established with respect to water, as was shown by the constant quantity of hydrogen evolved, when various acid, alkaline, and saline solutions were compared with one another, by passing the same electric current through them. I conceive, however, that in solutions of the hydracids, the acid is not directly decomposed; and that these cases merely afford additional instances of the definite decomposition of water. The diluted hydracid was placed in a tube connected with the negative side of the battery, and water, pure or acidulated with sulphuric acid, was placed in another tube connected with the positive side, the communication between the two liquids being established by moistened asbestus. In these circumstances, hydrogen and oxygen were evolved from the two poles, but no chlorine or iodine was drawn towards the positive side in either liquid; and it was only after a long time, when some of the hydracid itself had passed into the positive liquid, that chlorine or iodine appeared in very minute quantity by a secondary action. On the other hand, when the battery was reversed, and the hydracid made positive, and the water negative, the oxygen was no longer evolved, and the chlorine or iodine immediately appeared as a secondary product. Analogous experiments showed that the decomposition of the haloid salts when in solution was a secondary effect. I have not made any experiments on dry and fused substances, with the particular view of inquiring whether a definitive action of the electric current takes place in such cases; but several of Dr Faraday's experiments certainly appear to lead to that result.

EDINBURGH, *8th June* 1835.

Memoir on the Degree of Selection exercised by Plants, with regard to the Earthy Constituents presented to their absorbing Surfaces. By Charles Daubeny, M. D., F. R. S., L. S., G. S., Professor of Botany and Chemistry in the University of Oxford.*

Amongst the subjects recommended for consideration by the Chemical Sub-committee of the British Association for the Advancement of Science, during their meeting at York in 1831, was that of the sources from which organic bodies derive their fixed principles; and as it was known to some of my friends that I had been engaged in certain inquiries that bore upon this subject, a request that I would undertake the investigation was accordingly entered upon the minutes. I obtained, therefore, from this circumstance an additional motive for endeavouring, so far as my opportunities allowed, to prosecute the train of experiments which I had begun; and if I should scarcely yet have succeeded in determining to my entire satisfaction, whether or no there be any foundation for the idea sometimes entertained, that the earthy and alkaline principles which organized and living bodies contain are in any case elaborated by themselves, the reason must be sought for rather in the intricacy of the subject than in any want of disposition on my part to carry on an inquiry so recommended. Incidentally, however, the results of my researches seem to lead to the establishment of a fact, which, as it serves to modify one of the conclusions deduced by the younger Saussure from his experiments on vegetation,† deserves, perhaps, a brief notice; and it is on this account, rather than for the sake of any new light I may have been able to throw upon the principal point in question, that I am desirous of laying before the Society the following details.

In the experiments that were made by Braconnot,‡ Schrader,§ and others, with a similar intent to my own, the plants operated upon, in order that all external sources for the supply of earthy matter might be cut off, were made to vegetate either in washed sand, in sulphur, in pounded glass, in small shot, or in certain metallic oxides. It occurred to me, however, that without placing them under circumstances so unnatural, and consequently so unfavourable to growth, the same end would be fulfilled if the seeds were sown in some earth, which, though foreign to their constitution, agreed, nevertheless, more nearly in mechanical properties with those contained in the soils in which they were wont to grow. It was with this intent that I was originally led to select as a soil for my plants the sulphate of strontian, which is obtained in abundance near Bristol, reduced to fine powder: and having found that the ashes of plants which had been reared in this matrix seemed to contain no trace of the earth, I was led, in the next place, to try whether this might be owing merely to the insolubility of the substance in question; for which reason I varied the experiment by watering my plants with a weak solution of nitrate of strontian. It will appear from the subsequent details, that in either form of the experiment, lime, and not strontites, was the earth that presented itself; but as in proportion to the care that had been taken to exclude any external source of supply for earthy matter, the quantity obtained from the ashes grew less and less, it would be rash to infer, from the small excess of lime which was detected, any power belonging to the plant of forming it, when not supplied from without. Should it, however, appear that a vegetable, which, though not perhaps in full vigour, was at least in a growing and healthy condition, remained in contact with strontian, both in the state of sulphate, and likewise in that of nitrate dissolved in water, for months together, without absorbing any portion, and that, although in want of earthy matter, as its laxity of fibre evidently betrayed, the conclusion would seem to follow, that plants have, to a certain extent, the power, as living agents, of rejecting such substances as, without being poisonous, are unusual to them, and probably unfitted for their economy and structure. Omitting some previous experiments, of which I have preserved no correct notes, I will, in the first instance, refer to one made in 1827, in which grasses and trefoils of various kinds, which had been watered from time to time with a solution of nitrate of strontian, were found, on examination, to possess no trace of this earth. In the above instance, however, as the plants had grown in common garden mould, all that could be inferred was, that when lime and strontian are both presented in a state of solution to their roots, they select the former, and reject the latter. In 1829, the seeds of various plants, such as the garden raddish (*Raphanus sativus*), the cabbage (*Brassica oleracea*), the garden bean (*Vicia Faba*), hemp (*Cannabis sativa*), &c., were sown in soils containing various proportions of sulphate of strontian, with or without manure, and, amongst the rest, one in which no other ingredient except this earth, was present in any quantity. The plants grew up, and when they had arrived at maturity, were collected, burnt, and their ashes examined. No strontian, however, could be detected in any one of them, not even in that where the matrix consisted almost wholly of the earth in question. In 1831, the experiments were conducted with rather more attention to accuracy. 1124 grains of scarlet kidney-beans (*Phaseolus multiflorus*) were sown in a box containing about 290 lb. of powdered sulphate of strontian, which has been ascertained to be free from alkaline matter, but to contain 2 per cent. of carbonate of lime, and about ½ per cent. of alumina. The box was placed in an open situation, exposed to sun and rain; and when the plants reared from these seeds had come to maturity, they were cut down and burnt. An account was then taken of the weight of the ashes remaining after the combustion had been completed, and of the fixed principles obtained from them, first, by lixiviation in water; secondly, by digestion in nitric acid; and, thirdly, by treating the remainder with an alkaline carbonate, and then, again, with the same acid as before. A similar process was gone through with the same quantity of the kidney-beans as that of which the plants examined had been the produce.

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* Read to the Linnean Society November 19, and December 3, 1833; and printed for their Transactions.

† *Recherches Chimiques sur la Végétation,* 1804.

‡ *Annales de Chimie,* vol. lxi. p. 137.

§ Gehlen's Journal, vol. v. p. 255.

The following will present a tabular view of the results obtained.

Subject of the Experiment.	Where sown.	Weight of its ashes.	Soluble portion of these ashes.			
			In water.	In nitric acid.		
				Without previous treatment.	After having been acted upon by an alkaline carbonate.	
					Earthy phosphate.	Earthy sulphate.
Seeds of *Phaseolus multiflorus*, 1124 grs..........	....	106	6.7	0.67	0	0
Ditto, 1124 grs.	In a soil chiefly composed of sulphate of strontian, in a garden.	283	11.3	131.5	31.0	2.3

Now, the aqueous solution represents the amount of alkali combined either with the phosphoric or carbonic acids; the solution in nitric acid without previous treatment, the earthy carbonates and phosphates;* that in nitric acid, after the action of an alkaline carbonate, the earthy sulphate, with that portion of the phosphate which had escaped the previous action of the acid. These were not distinguished with any precision one from the other, because my object was merely to show that a large increase in earthy matter had resulted from the process of vegetation; but the several portions were all minutely examined for strontian, of which they furnished no trace. The same year I endeavoured to ascertain how much of this increase might be attributable to the rain, and the matters brought with it, by the following experiment: I procured six oblong boxes, of nearly equal size, coated internally with sheet-zinc, two of which were filled with sulphate of strontian, two with powdered Carrara marble, and two with sea-sand, well washed both with water and muriatic acid. Of these, one of each kind was placed in a greenhouse, where they were protected from dust and rain; and the same number in an open garden, where they were exposed to both.† There was also placed in the garden a fourth box, of twice the dimensions, filled only with common mould. In each of the six smaller boxes were sown 780 grains of the seeds of the winged-pea trefoil (*Lotus tetragonolobus*); in the largest one double that quantity; and when the plants had severally grown up in their respective situations, they were cut down, dried, reduced to ashes, and examined, a comparative analysis being at the same time made of a quantity of the seeds equal to that planted in each of the six smaller boxes. It will be seen from the following tabular view of the results obtained, that in every one of these cases there was an excess of earthy salt beyond that existing in the seeds, and in one case an excess of alkaline; those even which had vegetated in a soil chiefly consisting of sulphate of strontian obtaining, nevertheless, an increase of earthy matter, and this containing not even a trace of strontites, but consisting wholly of lime. In other respects the quantity of earth obtained appeared to keep pace with that in which the plant was supplied with it from without. Thus, the largest amount of lime was from the plants that had grown in Carrara marble, and of silex from those that had grown in sand. On the other hand, the great increase of calcareous salts in the produce of the seeds that had grown up in the garden, in a soil consisting of sulphate of strontian, indicates how much is owing to the quantity of earthy matter brought to it by the rains.

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* The difference in the quantity of lime to be inferred from 100 of phosphate and 100 of carbonate was only as 53 to 56.

† I am indebted to Professor Buckland for the use of a garden, in which the boxes were placed during the time the experiment lasted.

The following is a tabular view of the results of the above-mentioned experiments :—

Subject of the Experiment.	Where sown.	Weight of the plant when dried.	Weight of its ashes when burnt.	Portion soluble		Portion insoluble in these menstrua.
				In water.	In nitric acid,	
				Consisting of		
				Potass combined with carbonic and phosphoric acids.	Same combined with carbonic and phosphoric acids.	Chiefly siliceous.
Lotus tetragonolobus, seeds 780 grs. ……	………	…	30	5.2	3.4	only a trace.
Same quantity of ditto.	*In a Greenhouse.* Soil. Sulphate of strontian.	4002	60	very small.	17.15	not estimated.
Ditto.	Carrara marble.	2233	67.5	1.8	20.9	not estimated.
Ditto.	Sea-sand.	1135	34.3	4.1	6.0	not estimated.
Ditto.	*In a Garden.* Soil. Sulphate of strontian.	4862	94.0	0.72	27.2	not estimated.
Ditto.	Carrara marble.	3267	64.5	a trace.	28.2	1.5
Ditto.	Sea-sand.	2957	67.0	a trace.	16.0	8.8
1560 grs. of ditto.	Common garden mould.	10534	164.50	33.6	27.70	10.0
or 780 grs.	In ditto.	5267	82.25	16.8	13.85	5.0

In 1832 I made similar arrangements to those just alluded to, with the addition of a fourth box, containing washed flowers of sulphur, and the omission of those which the preceding year had remained in a garden. The four boxes employed being placed in a greenhouse under cover, 300 grains of barley were sown in each of them, and they were severally moistened, as they seemed to require, with distilled water, containing in every ten gallons two ounces of nitrate of strontian. The plants were treated in the same manner as on the preceding year, though, it is to be remarked, they did not thrive equally well. They were not cut down until the whole of the water had been expended upon them ; so that we may calculate about half an ounce of nitrate of strontian to have been applied to the roots of each.

The following is a tabular view of the results obtained :

Subject of the Experiment.	Where planted.	Weight of the dried plant.	Weight of its ashes.	Soluble portion of these ashes:				Portion insoluble in these menstrua.
				In water.	In nitric acid.			
					Without previous treatment.	After being acted on by an alkaline carbonate.		
						Earthy phosphate.	Earthy sulphate.	
Barley (*Hordeum vulgare*), 300 grs.	....	...	7.7	1.53	1.68 N.B. Consisting entirely of phosphate.	....	.45	2.04
	In a Greenhouse. Soil.							
300 grs. of ditto.	Sulphate of strontian.	383	61	13.3	17.0*	3.6	1.3	0.9
Ditto.	Carrara marble.	230	34	7.8	14.5	....	2.5	0.8
Ditto.	Washed sea-sand.	260	45	10.5	5.9	0.9	2.5	2.1
Ditto.	Flowers of sulphur.	78	7	0.9	4.0	none.	none.	0.1

At the same time at which the above four samples had been planted, 100 grains of barley were sown in flowers of sulphur, and moistened only with distilled water. This latter yielded only 16 grains of the dried barley straw, and being burnt, left no more than 1 grain of ashes,—a quantity so much less than what would have proceeded from the 100 grains of barley, of which it was the produce, that I thought it useless to carry the examination of them any further *.

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* These salts for the most part consisted of nitrates of lime derived from the action of nitric acid upon the earthy carbonate, of which the greater part consisted.

I may remark, that all the four samples of barley-straw, which had been watered with the strontian solution, were examined with care, in the hope of detecting in them the presence of that earth ; but the earthy matter obtained from those planted in sea-sand and in sulphur presented not even a trace of it, that from sulphate of strontian only 0.3 of a grain, that from Carrara marble only 0.4,—an amount beyond comparison smaller than what would have been present had it been secreted with the same readiness as a calcareous salt would have been. Yet that the presence of nitrate of strontian did in some measure contribute to the growth of the plant, may be inferred by comparing the amount of barley-straw obtained from the flowers of sulphur watered with that solution, and that from the same matrix moistened merely with distilled water. In the first case, the barley-straw weighed 78 grains, and the ashes derived from it 7 ; whilst in the second, that from an equal amount would have yielded 48 grains, and its ashes only 3 grains. The same year a similar train of experiment was pursued with the *Lotus tetragonolobus*, or winged pea trefoil. Six hundred grains of the seeds of this plant were sown in each of the boxes employed in the foregoing experiments. They were moistened from time to time, as before, with water containing two ounces of nitrate of strontian to the ten gallons, and they were not cut down until the whole of this water had been expended upon them. In order the better to arrive at an approximation to the actual increase of solid matter obtained during the process of their vegetation, the plants were taken up by their roots, and the adhering earthy matter carefully detached; but lest this should have been incompletely effected, the stems and other parts of the plant which had been above the surface of the soil were separated, so that these at least might be considered free from any ingredients, except such as constituted integrant parts of its actual composition. The several portions of these respective samples having been weighed, reduced to ashes, and examined in the usual way, the results were obtained indicated in the following Table, in which the stem, leaves, and flowers, are comprehended under the head of "Parts above ground;" the roots and seeds which had not germinated, under the head of those "under ground."

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* M. Laissaigne, as quoted by M. Richard, made an experiment to the same effect, and with similar results to this of mine. But his mode of conducting it appears in this respect unsatisfactory, inasmuch as the plant was taken up before it could be expected, in the natural course of things, to have begun to draw upon external sources for a supply of earthy matter. It is well known that the albumen of the seed is expressly provided for the nutriment of the infant plant ; hence, the first effort of germination is to produce nothing more than an evolution of matter previously existing in the seed, and it is only in the future progress of the plant towards maturity, after this internal supply has been exhausted, that we can hope to trace, if at all, any increase of earthy or alkaline matter. Now M. Lassaigne's experiment was stopped at the end of fifteen days, a period too short to admit of much accession of earthy matter from without to have taken place.—See Richard's Elements of Botany, English Translation by Dr Clinton, p. 213.

Subject of the Experiment.	Where sown.	Weight of the dried plant.	Weight of its ashes.	Portion soluble in water.	Portion soluble in nitric acid.
Seeds of the *Lotus tetragonolobus*, 600 grs.	……………	………………………	23	4.0	2.6
	In a Green-house.	*Parts*			
	SOIL.	Gr.			
Ditto. Same quantity.	Sulphate of strontian.	above ground … 170	22	6.35	2.65
		under ground … 107	40	0.48	5.60
		277	62	7.13	8.25
Ditto.	Carrara marble.	above ground … 150	19	3.70	16.3
		under ground … 152	34	3.20	37.0
		302	53	6.90	53.3
Ditto.	Washed sea-sand.	above ground … 34	5	2.3	1.4
		under ground … 100	6	1.4	1.7
		134	11	3.7	3.1
Ditto.	Flowers of sulphur.	above ground … 100	6	2.6	2.9
		under ground … 108	5	2.6	1.9
		208	11	5.2	4.8

The aqueous solution consisted chiefly of potass combined with the carbonic or phosphoric acids, together with a slight admixture of sulphate of lime, whilst the portion which the acid dissolved was chiefly composed of an earthy carbonate and phosphate. Now I satisfied myself, by a minute examination, that the acid solution derived from the stems contained no trace whatever of strontian, although a small portion appeared to be present in, or at least adherent to, the roots.

In other respects the results indicate decisively a connexion between the quantity of earthy matter contained in the plant, and the readiness with which it is supplied with it from without; since, even if we confine ourselves to the portions above ground, where there can be no suspicion of any foreign admixture, it will be seen that the largest amount of calcareous earth was obtained from the straw which had grown up in Carrara marble, and that the excess of it over that in the seeds was in the other instances but inconsiderable. The last experiment of the kind I shall allude to was made in the present year. Two boxes only were this time employed, the one filled with sea-sand, the other with Carrara marble. In each of them 500 grains of barley were planted; they were watered, as before, with a weak solution of nitrate of strontian, and were protected from dust and rain by being placed under cover in a greenhouse. The plants obtained, being burnt, were treated in the same manner as before, and rigorously examined for strontian. Of this, the roots of both samples appeared to contain a trace, though the largest amount did not exceed one-tenth of a grain. On the other hand, the parts which were above the surface, and therefore free from all contact with the soil, appeared to be entirely destitute of this earth. Nevertheless every portion, both of the sand and of the Carrara marble, was found impregnated with the nitrate of strontian that had been held in solution by the water with which the plants had been moistened.

I fear the conclusions that may be legitimately deduced from the above experiments will hardly be deemed of sufficient novelty and importance to repay the labour and time they have cost me; since, in so far as the main point is concerned, they serve only to confirm in an indirect manner the conclusion, which both analogy and experiment concur in establishing, namely, that if plants do in some cases obtain fixed principles which cannot be traced to any external source, yet the quantity of such substances which enters into their system is always less in proportion to the pains taken to cut off a supply. Hence the inference would seem to be, that the indications of a contrary description that sometimes present themselves are fallacious, resulting from the many imperceptible channels by which earthy and alkaline matter may obtain admission to the juices of a plant *. Had I not very early in the course of these experiments been led to despair of excluding the minute but continual supplies, which are probably brought by the very air and water which come into contact with the absorbing surfaces of every vegetable, especially in the centre of a large town, I should not have remained satisfied without purifying the sulphate of strontian in which the seeds were sown from the other earths with which I found it to be mixed. But the labour of getting rid of these ingredients seemed to be uncalled for with reference to the objects to which I found it necessary to confine my inquiries; since, even had I employed the earth in a state of perfect purity, and detected an excess of lime in the plants reared in it beyond that contained in their seeds, still I should not have been justified in inferring the actual generation of earthy matter, any more than I have felt myself to be from the similar result I obtained when flowers of sulphur were the matrix in which the plants had vegetated. The faculty, however, possessed by them of rejecting strontian, even when presented to the absorbing surfaces of their roots in a state of solution, would seem sufficiently substantiated; and an analogous circumstance may be cited in the animal kingdom, if I can rely upon an experiment which I made several years ago, that of confining some hens of the Guinea-fowl during the breeding season in a place where they could obtain no other earth, except some powdered sulphate of strontian, which they appeared to devour greedily. Yet only a minute trace of this earth was discoverable in the shells of their eggs, of which those laid during the first part of their confinement retained their natural hardness, but those of later production were as soft as if the

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* The case which I should be most disposed to bring forwards in support of the contrary opinion is that of the phosphoric acid, which forms so abundant an ingredient in all animal structures. Is its quantity sufficiently accounted for by that introduced into the system by the food taken in? On this subject I hope at some future time to complete some experiments. See also Dr Prout's paper on the phosphate of lime existing in the young chick before the egg is hatched.

birds had been entirely debarred from every kind of earthy matter. It may be asked, whether the strontian is taken first into the system, and afterwards excreted from it, or whether the spongioles of the roots refuse it admission. The latter supposition seems the more probable one, since, if we adopt the former, we ought to be able always to find traces of the earth diffused throughout the vegetable tissue; and I may relate an experiment of my own, which seems to confirm it, undertaken after the plan of those by means of which the ingenious M. Macaire of Geneva established his important doctrine with respect to the excretory function discharged by the roots of plants. A small *Pelargonium* was taken out of its pot, and its roots divided into two nearly equal bundles, one of which had its extremities immersed in a glass containing a weak solution of nitrate of strontian ; the other, in one containing pure distilled water. After a week had elapsed, the water contained in the second glass was tested ; but no strontian could be discovered in it, though a single grain in one pint of water would have been readily detected by my method. Hence it would seem that the strontian is not excreted by the roots. Yet this power of rejecting the earth in question, if possessed by the plant, must be held compatible with that of absorbing the water containing it, with which its roots are in contact. I took out of the ground a small lilac (*Syringa vulgaris*), and introduced its roots into a glass-globe containing seven pints of a weak solution of nitrate of strontian. In about a fortnight the quantity was reduced to three pints, the remainder having for the most part been absorbed by the roots ; for evaporation was prevented by covering the surface of the water with a stratum of olive oil, and the mouth of the vessel with a cork. Unluckily, the original quantity of salt had not been estimated ; but it was found that what remained in the water at the close of the experiment yielded 69.4 grains of sulphate of strontian, equivalent to 39.2 of the earth. The four pints of water therefore consumed, if they had passed through the organs of the vegetable charged with their original quantity of nitrate of strontian, would have carried into its circulation 22.4 grains of this earth ; and as the water was absorbed at the average rate of about 4½ ounces per diem, it follows that more than a grain and a half would have been carried daily through the substance of the plant, supposing the salt to have been taken up in the same ratio as the water. Now on burning the plant, and examining its ashes, a trace of strontian certainly was detected, but its whole amount did not reach the one-fifth of a grain, that is, 2 per cent. of the whole quantity of earthy matter present, my analysis indicating—Of lime, 7.30 grains; strontian, 0.18; total quantity of earth, 7.48.

The conclusion to which I have been led by the foregoing experiments may appear at first sight inconsistent with those deduced by M. de Saussure in his elaborate work on vegetation before referred to, in which he has shown that some poisonous substances, such, for example, as salts of copper, are freely absorbed by the roots of vegetables, and retained in considerable quantities in their tissue. But it will be recollected, that this philosopher himself accounts for the circumstance by the disorganization which such bodies, by their presence, occasion in the fibres of the roots. I have myself found that when a *Pelargonium* had a portion of its roots immersed in a solution of bichromate of potass, a trace of this salt was conveyed into a second glass containing distilled water, which had no connexion with the former, except through the medium of a parcel of the roots which dipped into it. Nor was this owing to capillary attraction, for the same effect did not take place in another experiment, in which the roots were detached from the body of the plant, and therefore acted as dead matter; and, moreover, the salt was detected by appropriate tests applied to the stems and leaves. In this instance, then, the substance was seen to circulate through the whole texture of the vegetable, and ultimately to be excreted by its roots; and a similar result was obtained in the case of another plant, in which a solution of proto-sulphate of iron had been dissolved in the water in contact with its extremities.*

But in all these instances the poisonous quality of the substance was evinced by the more or less rapid decay of the plant that had imbibed it; whereas, where nitrate of strontian was employed, the functions of life appeared to go on for a considerable time without material obstruction.

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* That is to say, the salt was detected by ferro-cyanate of potass in many parts of the stem and branches; but it did not reach above a certain point, nor was it excreted by the roots, this difference arising from the absorption of oxygen by the salt, which, being thereby converted into a persulphate, became insoluble in the juices of the plant, and consequently clogged up the canals by which the sap is conveyed.

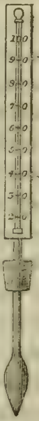
Upon the whole, then, I see nothing, so far as experiments have yet gone, to invalidate the conclusion, to which the preceding facts appear to lead, that the roots of plants do, to a certain extent at least, possess a power of selection, and that the earthy constituents which form the basis of their solid parts are determined as to *quality* by some primary law of nature, although their *amount* may depend upon the more or less abundant supply of the principles presented to them from without.

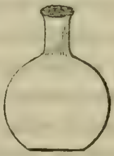
Note on Taking the Dew-Point directly, by the Method of Leroi and Saussure.

The following observations aim at nothing new in point of principle, and are merely intended to facilitate the long known and simple mode of finding the dew-point, by the cooling action of saline solutions on polished surfaces. The arrangement is by no means suggested as entering into competition with more costly and ingenious hygrometric instruments; but will probably be found useful in the absence of such aids.

The objects proposed are, first, to reduce within very narrow limits the quantity of materials, and size of the vessel used, and thereby extend the circumstances under which this method may be adopted; and, secondly, to avoid the trouble of any transference of the materials from one vessel to another, or of any successive additions of the substances in the course of the same experiment, by producing the reduction of temperature simply by a gentle and carefully regulated agitation of the vessel.

A little hollow ball, or bottle of thin brass, like that in the opposite page, is employed, having a diameter of $1\frac{1}{4}$ inch. The neck is $\frac{6}{10}$ to $\frac{7}{10}$ of an inch long, and about $\frac{4}{10}$ wide, and is farther expanded into a funnel shape at top. The bottle, when filled to the top of the neck, contains about $\frac{1}{2}$ an ounce. The outside is preserved bright and polished; and it will save trouble in cleaning, although not answer the purpose better, to have the outside gilt. A small thermometer is used, having a very fine capillary bore, and an oblong bulb $\frac{6}{10}$ to $\frac{7}{10}$ of an inch long, by $\frac{2}{10}$ wide; a very light and narrow scale, four inches long, and ranging from 100° Fahr. to near zero, is attached from $2\frac{1}{2}$ to 3 inches from the extremity of the bulb; and a cork capable of fitting into the neck of the bottle slides on the bare part of the stem. The more similar the circumstances, the more constant the results are likely to be.





As the cooling material, equal parts of nitre and sal ammoniac, are employed, the salts being rather finely pounded, well dried, and thoroughly mixed together; and a stock of this mixture is kept ready prepared in a close bottle. A little glass tube is provided, capable of holding, when filled to a mark, 40 grains of the mixed salts. This quantity is called a measure of the salts. Another tube is in like manner provided, holding, when filled to a mark, $\frac{1}{4}$ oz. measure. This is called a measure of water.

Suppose, then, that we introduce into the brass bottle one measure of the salts. If we next, taking care that the neck is not obstructed by salt, pour into the bottle very gently one measure of water, at the temperature of the surrounding air, and immediately, but cautiously, introduce the thermometer into the bottle, so that the bulb shall reach the bottom, and the cork fit into the neck, the mercury will fall a few degrees, rarely exceeding five or six; and the farther reduction of temperature we have entirely at our command. By an agitation of the bottle, at first very gentle and cautious, and afterwards slowly and gradually increased, holding it by the upper part of the neck, the farther fall of the thermometer will be so regular and gradual, that we can easily mark the temperature at which the deposition of moisture occurs on the polished external surface of the brass ball.

With the proportion of salts and water just indicated, we can usually obtain a reduction of temperature of from 15° to 20° below that of the water used. This will include by far the greater number of cases of the dew-point which will fall under our observation. By employing 1½ measure of the salts with the same proportion of water as before, a few degrees greater fall will be commanded; and with two measures of salt and one of water, a still greater cold is procured if wanted.

The point that chiefly requires attention is a moist state of the atmosphere, of which the operator can easily judge by the actual or recent fall of rain, and by the general appearances of the heavens. In such cases, it is necessary to use water of a temperature from 5° to 10° above that of the air, which is readily accomplished, simply by grasping the measure containing the water in the hand for a minute or two before pouring it into the brass bottle. In this way, the initial fall of the temperature is so much retarded, that we can easily, during the subsequent gentle agitation, mark the dew-point, even when differing little from the temperature of the air; and whenever we find that a too rapid fall takes place at first, we have always this method in our power as a corrective.

On the other hand, when very fine weather, a high barometer, and other general appearances, indicate a very dry state of the atmosphere, it may be proper to employ 1½ measure of salts and one measure of water, to ensure a greater reduction of temperature.

The usual mode of operating will, however, be with one measure of the salts and one measure of water, either of the temperature of the air, or some degrees above it, as appearances may suggest. After a little experience has given the requisite dexterity in the manipulation, and a moderate degree of foresight respecting the general state of the atmosphere, we shall not often find it necessary to repeat the operation. In a series of observations at the distance of some hours from one another, the preceding observation will of course be some guide to the subsequent *.

With the ball, thermometer, and measures, and a small bottle of

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* In a very few instances, when the dew-point is high, the layer of salt causes a deposition on the bottom of the bottle, before it extends to the sides. This appearance, which occurs only very rarely, is to be overlooked, because the thermometer is scarce affected by the cold which occasions it; and we must wait until, during the gentle agitation, we observe the deposition on the middle part of the sides of the bottle, as we are then sure that the cold has been equally diffused, and the thermometer duly affected.

the mixed salts, we are ready for finding the dew-point in all situations, and a very few minutes will suffice for the purpose. Within-doors, we have only to call for a jug of water, and a small cup to receive the liquid used in rincing out the brass ball once or twice after the operation. In the open air, a spring or brook will supply all our wants. In situations where water may not be found, as on the summit of mountains, a phial of the liquid carried with us will remedy the deficiency; and if its temperature has risen a little too high, we have only to employ 1½ or 2 measures of the salts, as may be necessary. The writer of this note has, for a year or two past, used this little apparatus in a great variety of situations, and, amongst others, during the barometric measurement of heights; and having thus had sufficient experience of its usefulness, he has thought that it might be worth while to give this short notice respecting it.

During frost, water at 33°, and 1, 1½, or 2 measures of salts, as circumstances may point out, will give the requisite cold. For more severe climates, the vessel might be of polished platinum, and the materials an acid and ice, or the proper salts.

AR. CONNELL.

SCIENTIFIC INTELLIGENCE.

NATURAL PHILOSOPHY.

1. *Velocity of Electricity.*—The following notice of Professor Wheatstone's very interesting experiments on *the velocity of Electricity*, will enable the reader to form a general conception of them. Two very great difficulties present themselves in this inquiry, the one arising from the circumstance that the sensation of light on the retina continues longer than the impression which produces it, as is illustrated by the well known experiment of a luminous circle being produced by a point of light in rapid rotation; this prolongation, very manifestly, is a great obstacle to the exact measurement of the duration of the impression: the second difficulty is the prodigious extent of any conductor that could be used for the purpose of exhibiting any appreciable difference in the transit of a spark from one point to another. The immense velocity of electricity makes it impossible to calculate it by direct observation; it would require to be

many thousands of leagues long, before the result could be expressed in the fractions of a second. It being thus impossible to arrive at any result by direct experiment, the Professor had to task his ingenuity that he might attain his object in some other way. After many fruitless attempts, he availed himself of the following expedient, which has already yielded many interesting results, and which he hopes still farther to improve, and render practically useful. He placed a double metallic mirror at the extremity of an axis of rotation, to which, by means of a large spinning-wheel, he could give an exceedingly rapid motion. The musical tone produced by the vibration of a card attached to the axis, supplied the number of revolutions. A luminous point, seen by the reflection of the two faces of the moving mirror, produced, in every semirevolution, two luminous circles, proceeding in the opposite direction to the mirror. All inequalities of the flame were made manifest by the circles appearing more or less distinct. By means of this arrangement, he discovered a series of luminous condensations and dilatations in the flame of hydrogen, when it produced sound in passing through a glass tube. He also observed that the electrical aura, as manifested in erecting feathers, down, &c. was not a continuous stream, but a rapid succession of very minute sparks. He then took a wire half a mile long, which he cut in the middle, and arranged each half in such a way, that both extremities and the central ends were in a line parallel to the mirror, so that the three sparks appeared on one straight line. As this sensation on the retina continued longer than the impression, instead of seeing points, which the three sparks should have produced, he observed three arcs of a circle, the origins of which should commence on the same right line, if the transit at the three sections were instantaneous. He imparted a velocity of 800 revolutions in a second to the mirror. Even at this degree of velocity, the origin of the first and third arcs continued on the same right line, but the middle one was somewhat in advance or behind the other two, according to the direction which was given to the rotation. Here the Professor remarks, that this experiment does not agree with the view that electricity is a single fluid, entering at the one end and issuing at the other, as Franklin's theory requires. As the sparks at the two extremities are always consentaneous, whilst that in the middle appears somewhat later, they indicate a double electrical cause, it may be that of two fluids, or a difference in the equilibrium manifesting itself on both sides at one time. He calculated the rapidity and the duration of the spark by its passage across half a degree of a circle of ten feet radius. The following are the conclusions which he draws: 1st, That the velocity of electricity through a copper wire one-fifteenth of an inch thick, exceeds the velocity of light across the planetary spaces; that it was at least 288,000 miles per second. 2d, That in breaking the electrical equilibrium in a wire communicating with a Leyden-bottle at two surfaces, the electricity appears at the two extremities of the wire at the same moment, and somewhat later at the middle of the circuit. 3d, That the light of electricity, in a state of great intensity, does not last the millionth part of a second. 4th, That the eye is capable of distinctly perceiving objects which present themselves for this short space of time.—This experiment is most deeply interesting for science; but it requires to be followed up and varied before comparative and positive results can be drawn. The author has pledged himself to do this, and his additional communications will be expected with much anxiety. We shall only remark, that many questions will demand a separate investigation. For example:—Does the velocity vary with the intensity? Does it vary with the conductors? Is it the same at all times in the same conductor? If electricity is propagated in the planetary spaces, is its velocity the same as along material conductors?—It can scarcely be doubted that, in electricity of feeble intensity, great differences are observed betwixt the velocity of propagation in the first and the following experiments. If we take a battery which has been for a considerable time in a state of repose, the electricity of feeble intensity will with difficulty, and only after a time, overcome the first inertia; but once overcome, quantities still more feeble will easily pass, either in the same or in an opposite direction. It is in electricity produced by heat that this inequality is most sensible. A similar effect takes place in the electricity produced by gold leaf. It is not after repose that there is the greatest sensibility, but when their primary inertia has been overcome. This circumstance should not be overlooked, either in experiments, or in theoretical conclusions respecting the condition of atoms.—*L'Institute.*

GEOLOGY.

2. *On the Imprint of the Feet of Animals upon Red Sandstone.*—A geological phenomenon has just been observed in Germany, which has much resemblance to one which was pointed out some years ago in Dumfriesshire in Scotland.* On the inferior surface of a bed of freestone, at the depth of 15 and 18 feet, in many quarries of Weikersrode, near Hildburghausen, have been observed inequalities or eminences which are evidently owing to the impression of some one's step, and very probably to the feet of some amphibious animal. The traces both of large and small animals are easily discovered, which appear to have been going, sometimes in the same, and sometimes in contrary directions. Invariably the print of a large foot is succeeded by another of less dimensions. The larger, which are about eight inches long, and four broad, have at first sight the appearance of a human hand, in which the thumb was placed very far back. Immediately underneath this freestone, is found a thin bed of bluish clay of considerable consistency, and which, without doubt, would receive, when still moist, the imprint of the feet of those animals which walked upon it. This clay would become hard whilst drying previous to the formation of the bed of freestone, and, beyond question, it was in these kinds of moulds that the impressions would be formed in relief, which are observed on the lower side of the upper bed. On this same lower side are also found a great many elevated furrows, crossing in all directions, like the meshes of a net, and which, in all probability, are owing to the cracks and fissures which would be formed in the clay, on account of the contraction which the drying would produce. The discovery of this interesting fact is due to Mr M. C. Barth of Hildburghausen, who first studied it with attention, and communicated it to Messrs R. Bernhardi and Gleichmann, who have recognised the closeness of the resemblances, and supplied the above as the most probable explanation. They have also thought that this sandstone should in consequence be referred to the new red, although its other

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* If the so-called foot-marks figured in the Transactions of the Royal Society of Edinburgh, are faithful representations of the appearances on the surface of the sandstone, no one will hesitate in saying, whatever they may be, they are not impressions of the feet of animals.

geological characters approximate it to a sandstone of a more recent formation; and they have promised on this curious subject more lengthened details, along with plates of the greatest accuracy.—*Neues Jahrbuch fur Mineralogie*, &c. 1834.

3. *Rolling of the Sea on the Coast of Peru.*—Nautical readers, it is remarked in the Quarterly Review, will probably like to read Dr Meyen's (author of the Voyage round the World) observations on one of those sudden changes to which the South American seas are frequently exposed. We believe this subject was first brought under consideration by Captain Hall. It is known that, on the coast of North Chili, as well as along the whole coast of Peru, an undulating movement of the sea frequently takes place, without any one being able to discover the cause; we ourselves have been lying during the night, and in the most complete calm, in the harbour of Capiapo, when the ship rocked so violently that we all found the motion intolerable. At other places, even south of Arica, we have, when the wind was perfectly still, seen waves thirty or forty feet high. It is known that, on the west coast of South America, the ebb and flood are very trifling, and, at a short distance from the land, quite invisible; so that, at the full of the moon, this phenomenon of the rolling of the sea, as it is called in these countries, cannot be ascribed to the tide. It has been attributed to the influence of the moon, and it is maintained that it only occurs at the full of the moon. But, in opposition to this theory, we can assert that this rolling, and in truth with the greatest violence, as for example in the harbour of Capiapo, took place during the last quarter, from which it may be concluded that the full moon is not the cause of it. On the whole, we are of opinion that the great flow of cold water, which sets in from the south-west, and touches the Peruvian coast in the breadth of Arequipa, must be regarded as the cause of this rolling of the sea.

4. *Large Mass of Native Copper.*—The cabinet of Yale College has been recently enriched by a magnificent piece of native copper. The donor mentions that this piece of copper was found at or near the river On-ta-naw-gaw of Lake Superior; and, as we are informed, above the rapids marked on the map of the river. Those who brought it away were allured by the vain hope of finding in it gold or silver. Its weight was 137 pounds, but is now somewhat less, as a few ounces have been

detached. It has all the character of native copper; the perfect colour and lustre of that metal; the occasional incrustation with green carbonate of copper; numerous rudiments of crystals of copper, with triangular faces; occasional cavities, swellings, and knobs, and great malleability. Its form is rudely planoconvex, with an irregularly elliptic base, arched below, and standing upon two projections, thus allowing it a rocking motion. It is fifteen inches long and fifteen broad; in the narrowest place twelve inches; it is nine inches high, and it bears strong marks of having been entangled in a veinstone, or a rock. Mr Catlin has been credibly informed, that there lies in the bed of the On-ta-naw-gaw, a mass equally pure, weighing a ton. Whether this is the celebrated copper rock described by Mr Schoolcraft, vol. iii. p. 205 of this Journal, we have no means to determine; but the numerous and important facts mentioned by Mr Schoolcraft, render it certain that native copper is frequently found in that region, and lead to a strong presumption of the existence of valuable mines of copper.—*Silliman's Journal.*

5. *Earthquakes.*—At present, as during our stay in the province of Santiago, certain minor earthquakes are regularly repeated every two or three weeks. A general alarm then seizes the inhabitants, and all desert their houses with loud cries of *Misericordia! Misericordia! il tiembla.* Some months afterwards, we found ourselves in the northern part of Chili, in the Partido de Copiapo, in a country where earthquakes rank amongst the most ordinary phenomena. Here the inhabitants were familiar with this dreadful curse; they sometimes remained the whole night within doors, whilst the houses were rocking and the trees waving to and fro. To such a degree can man accustom himself to the greatest danger.—*Meyen.*

6. *Fossil Marine Plants.*—The fossil marine plants of the family Fucoides, present an acceptable addition to our geological knowledge. The existence of marine plants in the region on the Juniata, was first announced, several years ago, by Dr Harlan. Mr R. C. Taylor, F. G. S., now confirms the existence of very extensive deposits of fossil fuci in the greywacke group of Central Pennsylvania. Several species have been observed in the brown sandstone of Tussey mountain, near Alexandria, in Huntington county, and farther south, in Bedford county. In the white sandstone of the Swan mountains in Centre Country,

the fossil fuci prevail 1700 or 1800 feet above the level of the sea. At Muncey Ridge, in Lycoming county, fine specimens were obtained in white sandstone; at this place and at Lewiston they occur 450 feet above tide-water; also on the eastern slope of the Alleghany ridge, fossil fuci are associated with productæ, at points more than 100 miles apart. In the narrows of the Junita, that river flows between cliffs of sandstone 700 feet high, and in the lower strata in the Shade mountain, the strata containing the marine plants are brought into view by the cuttings for the canal. They are found in shale, in sandstone, &c. In one place the fucus beds are laid bare to the height of nearly 50 feet, and here seven courses of the plants are comprised within a thickness of 4 feet. In another place, below Lewiston, eight or ten beds were counted within 6 feet. At the western end of Shade mountain, in a quarry, 100 courses of marine plants are distinguishable within a thickness of 20 feet; they are crowded with obscure plants, and occasionally crossed by the larger fucoides. In a third place, there are twenty layers of fucoides within the thickness of 3 feet, and it is supposed that the entire thickness is not less than 200 feet. At the west end of Shade mountain, the fucus beds extend, without interruption, to a height of from 300 to 350 feet; those containing the obscure algæ reach 250 feet, and at 300 there are numerous surface slabs, exhibiting the Fucoides alleghaniensis *in situ*. It is justly inferred by Mr Taylor, that there existed various epochs in which numerous surfaces of the shallows of the ancient ocean were covered by marine vegetation. The fucus beds are composed of argillaceous, slaty, and siliceous rocks, laminated and parted by shale. Some of the fuci had long flexible and flattened stalks, with few branches; the breadth of the stalks was sometimes over half an inch. On the western side of Shade mountain there are hundreds of beds, some of them not an inch in thickness, but making an aggregate of 200 feet, and the quarries within a mile of Lewiston, furnish an inexhaustible supply of excellent paving-stones, in which the vegetable forms, especially in the weathered specimens, are very prominent. This is perhaps the most remarkable locality of fossil marine plants that has any where been observed.—*Silliman's Journal.*

7. *Gold Region of the United States.*—A report has been

made upon this subject by James Dickson, F. G. S., London, by whom the fully ascertained Gold Region of the United States is considered as extending from the Rappahannock in Virginia to the Coosa in Alabama, while, at the same time, it is stated that gold has been found as far south as the Gulf of Mexico, and that it is probable it will ultimately be found to extend to Vermont, Canada, and perhaps the Arctic regions. Amidst failures and discouragements, fresh attempts and discoveries are making along the vast region bordering on the Blue Ridge, from Virginia to Alabama. Mr Taylor regards the Gold Region of the United States as richer than the similar regions of Brazil, Mexico, and Russia, while the security to persons and property, the abundance of food, the mildness of the climate, and the practicable surface of the country, present important adventitious advantages, not enjoyed in South America nor all of them in Russia. In Georgia, the richest mineral belt is in the talcose slate, and granite formations, alternating with hornblende-slate, gneiss, and chlorite-slate; parallel mineral belts are found also near Augusta, but they cease with the termination of the primitive region. The most productive researches for gold have been made in the branch-mines or stream-mines, in the beds of rivers, rivulets, and ravines. In such cases little capital is needed, and few machines except rockers, and the return is almost immediate and daily; from five to ten penny-weights per day for a single hand are not uncommon, and 120 have been obtained. In the loose deposits the gold is found in a bed of gravel, from 9 inches to 3 feet in thickness, and from 3 to 6 feet from the surface of the ground; it rests on slate, generally talcose, and is evidently the result of the destruction of a vein or veins crossing a watered ravine, or taking the same direction with it. Mr Taylor considers the process of washing as superior to that used in any country; the Burke rocker of North Carolina will wash a hundredweight (700 to 1000 bushels of gravel) a-day, and the machine costs, when complete, but 25 dollars. In working the trenches or pits of a branch-mine, numerous veins, partially decomposed, are to be seen in the soft bed of the talcose slate, where the superincumbent strata have been removed. The gravel strata are composed entirely of the broken fragments of the quartz veins, which are to be met with outcropping on the banks of the ravine. The ore itself, sometimes undecomposed, is met with in the bed, and all the characters of the mineral found in the vein are also to be met with in the branch gravel. The gold also is similar—for gold in some is entirely distinct in character from that of others. There was not a mine in Georgia, the gold of which could not be distinguished from any other of the same district, so distinctly marked were the characters of each. Branch mines have led to the discovery of many valuable vein-mines, for when they worked until the gold seemed to fail, they would come back and open into the sides or banks of the ravine, guided by the gold, and at last discover valuable bodies of gold-ore. Many instances of this kind are notorious in North Carolina and Virginia. The branch gold-mines of the United States are supposed to have yielded 6,000,000 of dollars, most of which is worked up in jewellery, and not in coinage. Three deposit-mines in Georgia have yielded 500,000 dollars, and Mr Taylor confidently anticipates that the gold-deposits of the United States will yield far larger returns than those of Brazil, Colombia, and the Urals united. The explorations for gold have not as yet been carried to a great depth, the greatest not exceeding 150 feet, and few of the shafts are over 100 feet, and most do not exceed 20 or 30. These excavations are too shallow to afford satisfactory information respecting the gold, and the digging is often abandoned upon the slightest unfavourable appearance, such as the narrowing of the vein, its dislocation, or its becoming shattered; for there is much appearance of disorganization in the veins and rocks. Pyritical ores constitute the mass of the ores in Columbia, Brazil, and the United States; above the depth of 100 feet they have been, in this county, partially decomposed; the yellow ores have been converted into brown, red and purple hydrates of iron, and a portion of the gold they contain having thus become uncovered, is accessible to amalgamation, while a large portion more is or can be developed by the essay by fire. Most of the gold is extracted by amalgamation, after stamping under water, and the residuum still contains gold. Messrs Andres Del Rio and John Millington, as a committee from the Geological Society of Pennsylvania, have investigated the Rappahannock gold mines in Virginia, situated on the river, about ten miles from Fredricksburgh; the tract is about 230 yards wide, by an average length of upwards of 900 yards. The metalliferous veins consist of hard quartzoze rock between walls of decomposed talcose slate. A portion of loose red soil, by washing two handfuls of it, gave a considerable quantity of minute granular gold, and similar results were obtained by washings in other places. A principal auriferous quartz vein is from 2 feet 6 inches to 3 feet 6 inches wide; it stands vertically between walls of talcose slate; there is also, on either side, a vertical bed of auriferous red earth, from 2 to 3 feet wide, and bounded also by talcose slate. The auriferous quartz vein has been exposed to view for 627 feet, with a width of 30 inches, and it would appear that this is only the beginning. By a rough process of washing, amalgamation and evaporation of the mercury, three and half grains of gold were obtained from four pounds of the ore, taken indiscriminately from all parts of the vein, and in another experiment five grains of gold were produced from four pounds of pure milk-white quartz, which had no appearance or indication of containing any metal at all. Messrs Del Rio and Millington think that each pound of the ore may be made to yield one grain of gold, or 5 pennyweights to the 100 pounds of ore; this would much more than pay the expense, which cannot exceed one dollar on 100 lb. of the crude material. It appears, that by heating the quartz red hot, and throwing it into cold water, 8 grains of gold were obtained from 5 pounds of ore. In the opinion of Mr Dickson, the Rappahannock mines perfectly resemble all the others in Virginia. On the whole, the gold region of the United States is very extensive, rich and promising, and there is every adventitious advantage of fuel, food, climate, cultivation and security. [We have seen a decisive experiment of this kind upon white quartz from Virginia, which yielded a considerable quantity of gold by simple pounding and washing without amalgamation.—Ed.]—*Silliman's Journal.*

ZOOLOGY.

8. *On Aphis Persicæ.* — M. Morren has presented to the Academie Royal de Sciences of Brussels, the second part of his memoir on the flight, in 1834, of the *Aphis persicæ.* He recalls to recollection that, during the autumn of 1834, various towns

of Belgium were visited by a kind of hurricane, in which was conveyed a host of small insects belonging to the species *Aphis persicæ.* The emigration appeared to commence between Bruges and Gand, and from this place, as a centre, extended to the north, the east, and the south. It is attributed by the author, not to the appearance of the cholera, as had been pretended, but to the extreme drought of the season, as it was also promoted by the prodigious fecundity of the females, and the rapidity of procreation, a single individual being able, as early as the second conception, to produce not less than 10,000. It is the anatomical structure which has especially excited the attention of M. Morren, so that, if possible, he might detect how it happens that the females in this genus will produce their young ten or eleven successive times, without intercourse with the male.

M. Morren states that he has discovered a very simple digestive canal, without any salivary or biliary vessels. Upon the inferior wings he has discovered a hook which unites these with the superior ones. The generative apparatus of the male is composed of four or five testiculi, inclosing spermatic animalculæ. The conducting canal is supplied with a large vesicula seminalis, much larger than any author has pointed out as belonging to the Aphidiens. The corresponding apparatus in the female is formed of an ovary of eight *ovi* or *fœti-gerens* sheaths, according to the season. These sheaths have each three or four apartments, where the young are gradually developed, and M. Morren describes and represents all these transformations. When in the condition of eggs, they are seen in the terminating apartments. There is no sebific gland. Thus the animal is not hermaphrodite; and it will not be supposed to be the product of spontaneous generation; whilst it was not previously known that the influence of the male could be transmitted through ten or eleven successive births. M. Morren, accordingly, believes that there is an individualization of organized matter in the individuals who possess the construction of this species; and this amounts to a kind of *equivocal generation.* The small scales on the abdomen of the *Aphis*, which help in the secretion of a saccharine matter, which the ants lick off, form also a part of the respiratory apparatus, at the same time that they lead to particular glands. This fluid is the nourishment of the young ones in their earliest stage of being; so that, according to M. Morren, the insect is a mammalia, according to the most accurate signification of the word. The *Dorthesics*, among the hemipteres, represent the Marsupiæ, as this fly represents, in the same class, the mammalia.

9. *Edible Birds Nests, how formed.*—There is a peculiar kind of bird-nest abounding on the Philippine Islands, which is in high request amongst Chinese gourmands. Mr Trelawney, in his Adventures of a Younger Son, tells us that the price of a moderate cargo is occasionally immense, and relates an amusing story of an English captain, who threw overboard enough of them to have made the fortune of his family. Dr Meyen thus explains the precise composition of this luxury:—The weed which composes this branch of commerce is *Sphærococcus cartilagineus var. setaceus aq.*, which is found in great abundance in this part of India. It is eaten by the bird (*Hirundo esculenta*), which builds the nests in question, and is used in the preparation of its precious nest. The swallow eats the fresh weeds, and permits them to soften for some time in its stomach, after which it throws up the mass, now converted into a jelly, and sticks it together to form the nest. The nests, which are subsequently smeared over with dirt and feathers, are brought in their raw state to China, where they are cleaned in immense warehouses built for the purpose, and then exposed for sale. These so-celebrated Indian nests are, therefore, hardly any thing more than the softened *Sphærococcus cartilagineus*, which we have brought with us from the Chinese seas, and their effects is no other than that of fine jelly. In the preparation of these nests, such a number of fine stimulants are generally added that they of right occupy the first rank amongst relishes at the tables of the Chinese. The Japanese had long ago discovered that these costly bird-nests are nothing more than softened seaweed, and now prepare the substance itself in an artist-like manner. Some of our own epicures may be glad to learn that the *Sphærococcus crispus*, which Dr Meyen thinks would serve just as well for the composition of this luxury, is to be found in large quantities on the western and northern coasts of Great Britain.

10. *Pilot-fish.*—The pilot-fish, says Dr Meyen, swims constant-

ly in front of the shark. We ourselves, says he, have seen three instances in which the shark was led by the pilot. When the sea-angel (query, devil ?) neared the ship, the pilot swam close to the snout, or near one of the breast-fins of the animal ; sometimes he darted rapidly forwards or sidewards, as if looking for something, and constantly went back again to the shark. When we threw overboard a piece of bacon fastened on a great hook, the shark was about twenty paces from the ship. With the quickness of lightning the pilot came up, smelt at the dainty, and instantly swam back again to the shark, swimming many times round his snout, and splashing, as if giving him exact information as to the bacon. The shark now began to put himself in motion, the pilot showing him the way, and in a moment he was fast upon the hook. Once we watched a pilot for many days who kept constantly swimming close before the keel of the ship. The sailors say, as of a thing well known and familiar, that such a fish, so situated, has lost his shark, and is seeking another. Upon a later occasion, we observed two pilots in sedulous attendance on a blue shark, which we caught in the Chinese sea. It seems probable that the pilot feeds on the shark's excrement, keep his company for that purpose, and directs his operations solely from this selfish view.

11. *Effect of Mountain Air on Man.*—One of the most interesting expeditions, says Dr Meyen, was one to the mountains of Peru ; a service of considerable danger, on account of a complaint which almost invariably attacks the traveller during the ascent. The symptoms are described as follows:—We were tormented with a burning thirst, which no drink was able to assuage ; a slice of water-melon which we had brought with us was the only thing we could relish, whilst our people ate garlick and drank spirits, maintaining that this was the best way to guard against the effects of the journey. We kept on ascending till two o'clock in the afternoon. We were already near the little ridge which extends W. S. W. from the summit of the mountain (the volcano of Arequipa), and we could even distinguish the little stones upon the summit when our strength at once abandoned us, and we were overtaken by the disease *sorocco*. The nervous feverishness under which we had suffered from the first, had been gradually becoming worse and worse ;

our breathing became more and more oppressed; fainting, sickness, giddiness, and bleeding at the nose came on; and, in this condition, we lay a considerable time, until the symptoms grew milder from repose, and we were able to descend slowly.

12. *Temperature of Fishes.*—Dr Davy, in a paper read before the Royal Society of London, stated that he had occasion to observe, many years ago, that the bonito (*Thynnus pelamys*) had a temperature of 99° of Fahr., when the surrounding medium was 80°5, and that it, therefore, constituted an exception to the generally received rule that fishes are universally cold-blooded. Having found that the gills of the common thunny of the Mediteranean Sea (*Thynnus vulgaris*, Cuv.) were supplied with nerves of unusual magnitude, that the heart of this latter fish was very powerful, and that its muscles were of a dark colour, he was led to conjecture that it might, like the bonito, be also warm-blooded, and this opinion is corroborated by the testimony of several intelligent fishermen. The author endeavours to extend this analogy to other species of the same family, which, according to the reports of the fishermen of whom he made enquiries, have a high temperature, and in whose internal structure he noticed similar peculiarities, as in the thunny; namely, very large branchial nerves, furnished with ganglia of considerable size. In this respect, he considers that in these fishes the branchial system of organs make an approximation to the respiratory apparatus of the mammalia, and that it probably contributes to the elevation of temperature, resulting from the more energetic respiration which he supposes to be exercised by these organs. He, however, thinks it not improbable that these fish may possess means of generating heat peculiar to themselves, and of which at present we have no adequate idea. He conceived that the situation of the kidneys, of which a considerable portion is even higher than the stomach, and posterior to the gills, and which are of large size, and well supplied with nerves and blood-vessels, may possibly act a part in the production of an elevated temperature; but, on the whole, he is disposed to ascribe the greatest share of this effect to the superior magnitude of the branchial nerves.

13. *Salmon of the Columbia River.*—Dr Gairdner, in a letter, dated Fort Vancouver, November 1834, says:—" I have

ascertained already the existence of *six* distinct species of true salmon in this river; five of these I have seen and preserved; the periods of spawning of each are different. From what information I have collected regarding their habits, this is the country to study this singular fish. It is found at the very sources of the Columbia, notwithstanding the apparently insuperable rapids and cataracts which must be passed. Almost every where the natives assert that the fish which ascend the stream never return to the sea, nor were the young salmon ever seen to descend to the ocean. This last is certainly incorrect, and must arise from the fry being still so small as to elude the observations of the natives. The former is not unlikely, from the circumstance of the salmon, in the months of November and December, being found at the heads of all the streams dying by thousands, and completely choking up the current with their bodies. They have often been seen with their noses fairly worn down to the bone, and in the last stage of emaciation; yet still, by some unaccountable impulse, striving to ascend the stream to the very last gasp. It is singular that the salmon pass by some of the tributary streams in their passage upwards, and prefer some of these to others. Few or none, for instance, are ever got in the Kowalitit and Kanagun, or Deasis rivers. They seem to delight in those streams where their progress is impeded by rapids and cascades; and it is remarked that, in Fraser's River, no sooner have they emerged from the rapid waters of the main stream, into the still waters of Stuart's Lake, and the other lakes, than they become flabby and of inferior flavour. The muscular power of this fish is truly astonishing, even in a class of the animal kingdom remarkable for the vigour of their movements; for they are seen to ascend channels at the Kettle Falls, into which a stone as big as a man's head, when dropped, is shot downwards with the swiftness of an arrow, and where it is impossible by any force to push a pole even to an inconsiderable depth.

14. *Respiration of Birds.*—In a paper read to the French Academy of Sciences, by M. Emili Jacquemin, the author comes to the following conclusions: 1. Organized beings exhibit three principal modes of respiration. The first consists of a simple and simultaneous gaseous exhalation and absorption,

gentle and continuous; the organized portion remaining passive and motionless; this mode of respiration is seen in plants. In the second, there is established an excessively rapid attraction and repulsion between the organized parts and the surrounding medium, whence a vibration results on the edge of the respiratory organs. Such is the respiration of a great number of the Infusoriæ, the Vorticellæ, the Kalpodes, the Leucophylles, the Rotiferæ, &c.; and amongst the animals of the radiata, the Plumatelles, the Acalephes; among the Mollusca, the Planorbes, the Lymneæ, the Uniones, and the Anodontes. In the third mode of respiration, the reciprocal action of the organized parts and the surrounding medium goes on in determinate intervals, separated by a moment of repose. Such is the respiration of the higher animals.—2. In birds, the air penetrates not solely into the lungs, and about the parietes of the chest; it also enters by foramina not very determinate, into eight pneumatic pouches, which occupy a large portion of pectoro-abdominal cavity.* Thence it penetrates into subcutaneous cellules, by means of the subscapular and subfemoral pneumatic pouches, and through the apparatus already described into the superior and inferior extremities.—3. The pneumatic pouches are so placed, that they can easily conduct the air into the more solid parts of the body, and may in this way surround the heaviest viscera, so that it supports birds during their flight, and thus contributes to facilitate their aerial locomotion.—4. The large quantity of air which thus finds its way into the most internal parts of their bodies, tends to dry the marrow in the interior of the bony cavities, and also a portion of the fluids which it encounters in its passage; hence results a diminution of the specific gravity of birds, the true cause of which has in vain been sought for in the quantity of air alone which penetrates into their bodies.—5. In birds, the oxidation of the nutritive juices is effected not only in the lungs, but is much promoted also in the pneumatic pouches. The air contained in them operates, through the membranes, upon the sanguiferous and lymphatic vessels with which they are in contact; hence there follows a far more complete and speedy oxidation.—6. Not the skeleton only, but all the viscera also, which go to form the body of birds, are much more permeable to air than they are found in any other of the vertebratæ. —7. The reservoirs for the air are not always symmetrical; their form and extent depend entirely upon the form and reciprocal situation of the organs among which they are placed. All that seems to be fixed is, that the total quantity of air received in these pneumatic pouches, on the right side of the body, is equal to that received on the left. Unless this condition were maintained, flying would be impossible, and walking would be difficult.—8. No portion of the structure of birds, including even the last phalanges of the wings and feet, and the last caudal vertebræ, is impervious to air. The quill of the feathers makes no exception to this, as has sometimes been alleged.—9. The air within the head has a distinct circulation, and does not communicate directly with the air-passages of the rest of the body.—10. In no part is the air in immediate contact with the viscera or the nutritious juices; it is invariably only through a membrane, how fine and transparent soever it may be. This fact would go, by analogy, to establish, that the air filling the lungs, even to the last ramifications and ultimate cells, comes in no other way in contact with the blood, in its ultimate vessels, than through a fine and delicate membrane, as thus happens in birds.—11. The large quantities of air which birds can thus introduce into their bodies, and the force with which they can again expel it, is the only consideration which can explain how so small a creature as the nightingale, for example, can produce notes so strong, and can sing so long and so delightfully, without any apparent fatigue.

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* The *air-cells* of English anatomists.

15. *On the Mytilus Polymorphus, or the Driessena Polymorpha, a remarkable species of mussel.*—M. Van Beneden, Conservator of the Natural History Museum of the University of Louvain, has presented to the Académie Royal de Sciences of Brussels, a memoir concerning the anatomy and natural history of the Driessena polymorpha, a new genus in the family of the Mytilaceæ. Pallas was the first who, in his Voyages, discovered this mussel in the different rivers of Russia, and also in the Caspian Sea. The great variety of forms under which it appeared, did not escape the observation of this naturalist, who designated it under the name of *Mytilus polymorphus.* That a true mussel should inhabit both fresh water and the sea, appeared so extraordinary to Lamarck, that he did not hesitate to regard this opinion of Pallas as erro-

neous. At a later period, it was discovered by Chemnitz in the Volga, and he, probably ignorant of the description by Pallas, described it under the name of *Mytilus Wolgæ.* * A long time after this, M. de Ferrussac, recognising the identity of these two species, united them in one, under the name of Mytilus Chemnitz. † But this was not all; for during this period it was alluded to by many authors, under the name which Pallas had given it, and described by others afresh under still different appellations. According to Van Beneden, this mussel is found throughout the whole of Europe, and America has specimens which very nearly approach to it. It inhabits the sea, also lakes, rivers, marshes; and all these positions appears to be equally favourable for it. Probably it is the only example in the history of mussels of an individual inhabiting so many different places and situations. The author communicates the result of his anatomical researches regarding it, which, in conclusion, he compares with the genus *Mytilus.* The name of *Driessena*, which he gives it, is derived from the name of M. Driessens, an apothecary of Mazeyk, from whom the author received, at the end of the year 1822, a collection of these mussels alive, which he found in a canal which was fed by the Meuse, and which ran between Maestricht and Bois-le-duc. ‡

16. *Discovery of Remains of the Mammoth in India.*—In the number of the Journal of the Asiatic Society of Bengal for August 1834, there is an account by Mr J. Prinsep of the discovery, in the valley of the Nerbudda, by Dr G. G. Spilsbury, of the right and left thigh bones of the Elephas primegenius or mammoth, which belonged to an individual 15 feet high. In the same work it is stated that remains of a fossil buffalo, the first time such remains have been met with, were found in the banks of the same river.

ETHNOGRAPHY.

17. *Projected Expedition to Iceland and Greenland.*—M. Parisit lately read to the *Académie Royal de Medecine* of Paris, a

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* Chemnitz, Conchilien Cabinet, xi. 256, lib. 205, f. 2828.

† Preserved in manuscript; and indicated under this name in many collections.

‡ It was found by Mr Stark two years ago in the Union Canal, near Edinburgh, and also by English collectors in some of the Docks at London. The British animal is conjectured to have been imported from abroad.

Report from a Commission who had supplied Instructions to Dr Gaymard, the physician of the expedition, sent in search of *La Lilloise*.—The expedition has two principal objects in view: the one to visit Iceland, the other to endeavour to effect a passage across the icy barrier, which has rendered Greenland inaccessible for nearly four centuries, and accordingly the commission directs attention to both these objects.—If the expedition reaches Greenland, the following objects of investigation are particularly indicated as deserving regard: Whether the Esquimaux are the true indigenous inhabitants of the country? Whether the wandering tribes have a close resemblance, or differ much among themselves, as it regards their external form, their moral energies, &c., a difference which was observed by Caillié respecting the wandering tribes in Africa? Are they all possessed, or is it only some of them, who have the incisor teeth enlarged, much like the molar, as is found in some of the ancient mummies of Egypt? Have they the caruncula lachrymalis of the eye covered with a vertical membrane? Are their eyes approximated toward their nose, as in the Chinese? and their feet extremely small, as in the Kamtschatkans? also, Have they beards? It should be examined if the Greenland Esquimaux have particular affinities with the Esquimaux scattered over the whole of the northern parts of America, and also with some of the tribes in Asia; and finally, as asserted by Robertson, with certain of the nations of Europe? Or do they form, as Blumenbach maintains, an intermediate race between the Mongols and the native Americans? It should also be ascertained whether the Esquimaux obtain the principal part of their food from the flesh of the cetaceæ and of fishes, and whether it is their oil which they chiefly drink. It should also be observed what influence this kind of nourishment has upon their general health; if whilst they may appear tolerably stout, the Esquimaux are notwithstanding but a feeble race, and but imperfectly nourished by this half digested oil; are debilitated and of a relaxed fibre; also whether this oil has any influence in producing the dark colour of the skin which is common among them.—Dr Gaymard is also directed to bestow attention on the inveterate cutaneous diseases to which they are liable, and ascertain their relations to the affections of the skin in temperate climates; he will particularly inquire if the scurvy exhibits itself under its accustomed form, and what are the remedies they find most efficacious against it.—There are many other objects which will demand inquiry. Thus it is said that from the surface of rivers and of lakes, a vapour arises, which in the air forms a nearly transparent cloud of particles of ice, so minute and sharp, that, carried along with the wind, and drifting over the soil, it overwhelms the Esquimaux and kills them with cold, somewhat as the clouds of burning sand raised by the Camsin in the desert, knocks the suffocated Arab to the ground. It should be inquired if there be any thing poisonous in these icy vapours, and if danger may be averted by falling flat on the ground, and half burying the face in the earth?—As it has been stated that the Esquimaux, in their long and keen pursuits after food, are liable to violent bleeding from the nose, and that their remedies are somewhat peculiar, these should be investigated. Thus it is said that they are in the habit of using suction at the back of the neck, and a tight ligature round the ring fingers of both hands; that they also fill the mouth with pieces of ice, and snuff sea-water up the nostrils. The efficacy of these or any other remedies will be ascertained.—In spring-time the brightness of snow is very trying to the Esquimaux; they are very liable to an ophthalmia, almost as bad as the famous Egyptian ophthalmia, and also to cataract. It will be ascertained if this prevalent ophthalmia arises simply from the strong light; and regarding the cataract, as it has been stated that the old women among them are almost always successful in couching with a needle, and extracting with a knife, the steps of their operations, and their after treatment should be ascertained.—Dr Gaymard will also pay particular attention to the different diseases to which the Esquimaux are subject, amongst which he will not forget the pleurisy, which they were wont to treat by the application of a tractor to the pained spot. He will learn what efficacy this has, and inquire if pleurisy is ever infectious here, as it evidently appears to be in Iceland.—In the department of natural history, it will be necessary to verify all the statements made about those northern bears, whose weight is said to mount up even to 1600 pounds; and regarding the Greenland shark, whose muscular fibre, it has been stated, contracts even three days after death, and concerning that peculiar appendix of the eye, which is alleged to be peculiar to it, and which seems to enfeeble its powers of vision; not forgetting the river swallow, which in Lapland, according to Regnard and numerous authors, passes the winter, under the water, in a state of absolute congelation, and is resuscitated by the first heats of spring.

This terminates the list of inquiries regarding Greenland. The expedition will probably continue a longer time in Iceland. The following are some of the instructions given regarding it.—This spot, peopled by a Norwegian colony, has been remarkable in very remote times for the taste it has displayed, we might call it a national passion, for the study of history. Hence those sagas or annals written so many ages ago in the Scandinavian tongue, and in which some contemporary nations, and especially the English, have found the original traces of their old laws. If these documents had been examined in a medical point of view, they might be found to contain some valuable hints for the science, and Dr Gaymard will keep this in his recollection.—Many phenomena occurring in the island connected with natural philosophy and geology, have hitherto been most imperfectly observed. There are a great number of great caverns in it, which are said to have been inhabited by giants, a report which possibly owes its origin to large fossil bones having been found in them. Respecting mineralogy the attention of the party is directed to the numerous sulphur springs which abound, and to the green lakes, the bottom of which is sulphur; also to the petrified wood, crystals, and the transparent lava of Krabla, which is cut without any preparation, to make optical instruments. It will be important to determine on the spot the composition of the numerous and varied mineral waters, their temperature, and their effects both on man and the lower animals.*—Olafsen says that certain of its plants have the power of fattening horses, though it somewhat weakens them, as also cows, though it diminishes their milk. When forage fails, the inhabitants are in the habit of giving the cattle the flesh of a fish, the *blennius* (Linn.) for food, mixed with the bruised bones, &c. of the cod-fish. It is stated that cows are very fond of this, and that it produces a great quantity of milk, which, however, has an unpleasant flavour. The qualities of this milk and its action upon the animal economy will be examined, and it will be also considered if chemistry cannot suggest some means by which its disagreeable taste may be removed.—Formerly the island was rich in flocks of sheep, but these were greatly reduced by a fearful epidemic of ten years' duration from 1750 to 1760. Inquiry will be made as to the nature and origin of this disease. A Swedish nobleman discovered a powder, which was very successful as a cure, and which is still employed in Denmark and Norway. The composition of this remedy will, if possible, be discovered.—When the flocks are shut up during the winter in the caverns, they sometimes suffer so prodigiously from hunger, that they eat the wool from their backs. In consequence of this, a sensation of weight is produced in the stomach, which much mitigates the uneasiness of hunger, and this expedient seems to be frequently had recourse to by all animals under similar circumstances. Hence those extraordinary balls known under the name of *ægagropiles*. The islanders are acquainted with a method by which they rid the sheep of these balls, and it will be important to learn what these means are. It will also be ascertained whether the sheep have four or five horns; if these drop off in their growth; and also if it be true that the Iceland dogs never bark, but only growl, as in Greenland.—A number of inquiries suggest themselves, which have especially to do with the medical art. The first complaint concerning which inquiry should be made, is that which was known under the name of *Digerdoïd*, and which, according to M. Ibre, constituted the great plague of the fourteenth century. Inquiry will be made concerning the direction from which it penetrated into Iceland, where it again manifested itself in 1402 and 1404, and where it has never since been seen.—The leprosy of the island, if we believe the characteristics given of it by authors, resembles neither the leprosy of Greenland, nor that described by Moses, which in fact is scarcely ever met with in the East, nor that of the Greeks, nor that of the Crimea, so accurately described by Pallas, and which has propagated itself to Astracan. It would be desirable to know if it were analogous with the malady of the province of Austurias in Spain, or, as M. Bœck thinks, with that of Martigues, and especially with that of Syria and Egypt. But it is probable that this disease, common to so many nations and different climates, depends on some cause which is common to them all. In Iceland it has been attributed to the immoderate use of fish, and to fish that was not fresh; and Peterson has made the remark, that it has vanished from the isle of Feroe since the inhabitants have addicted themselves more to agriculture and less to fishing. Opposed to this theory is the statement that, previous to the time of the Crusades, the disease was never known in the island, and that it was by these it was brought into the whole of Europe, whence it found its way into the island. Were this true, it would owe its naturalization in Iceland to the human constitution, being peculiarly prepared for its reception. At the same time it must be admitted, that leprosy exhibits itself under such numerous aspects, or rather perhaps is so often confounded with other diseases, that its history is still most confused and unsatisfactory. Hence the necessity of the most accurate inquiry,—if it be hereditary in the island, as elsewhere, and to what extent; if it appears early in infancy, and if there is an age when people are safe from its ravages; also as to the manner and the success of the treatment; and finally, if there be any examples of its disappearing before the course of the smallpox.--Similar questions occur regarding scorbutus, and need not therefore be detailed. Among the epidemic diseases it will be particularly important to attend to the *Land-farfoot*, a species of infectious pleurisy, equally formidable to children and the aged, but a sure token of longevity to the youth who recovers from its attack. A similar epidemic was observed at Dijon by M. Maret in 1784. With regard to pulmonary consumption, there are some authorities who state that it occurs in Iceland; others deny this. This controversy should be examined, and put an end to.—Every thing regarding the smallpox, vaccination, measles, croup, &c. will require minute attention; also if it be true, as stated, that scrofula does not exist in the island, and that rickets was not known there till within a very recent period. With regard to the islanders themselves, it is said that, in spite of the purity of the air and the salubrity of the climate, they are a feeble race, liable to disease, with little vivacity, seldom attaining fifty and sixty years of age, and scarcely ever eighty. It is the women, and especially those who have had a large family, who are alone reported to attain to old age. It ought to be ascertained if this feebleness of constitution is the result of their diet, or of their habits of life. It is the practice of the Iceland mothers to suckle their children only for three days, and then to stuff them with indigestible meats. Inquiry will be made as to the consequences of this, and concerning the common diseases of infancy among them. In the Westerman Islands, great numbers are destroyed by tetanus. The population is rapidly diminishing.—Finally, it will be investigated whether it is true, as certain travellers have stated, that the ices are increasing round the island; and that the sterility of the soil is augmenting; that the fish themselves are diminishing, and all the means of subsistence are decreasing, so that, sooner or later, man, thus banished by nature, will cease to leave his foot-print on the desert soil, and, in his absence, the ice and volcanic fires will dispute for the mastery.—Such is an epitome of the instructions which the Commission proposed to put into the hands of Dr Gaymard. Several additional inquiries were subsequently suggested: if the Greenland dogs were subject to hydrophobia; if cretinism was known in the country; what modifications the climate had induced in the manners, and perhaps even in the organization of the men and animals, who had originally sprung from an European origin; what is the cause of the intellectual and moral inferiority of the Greenlander in comparison of the European; whether owing to some defect in physical organization, or the result of ignorance and sheer misery.

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* Lapeyriere affirms that some of these spring waters, without any other remedy, cure syphilis. This should be inquired into.

Description of several New or Rare Plants which have lately Flowered in the Neighbourhood of Edinburgh, chiefly in the Royal Botanic Garden. By Dr GRAHAM, Prof. of Botany.

Dyckia.

GENERIC CHARACTER.—Calyx 3-partitus, subcarnosus, rectus, adpressus. Corolla 3-fida; segmenta subæqualia, spathulato-rhomboidea, erecta, nunquam spiralia. Stamina 6; filamenta lanceolata, tubo corollæ inserta; antheræ sagittato-oblongæ. Germen superum, pyramidali-oblongum.

D. rariflora; foliis liniari-subulatis, recurvo-patentibus; spica rariflora; bracteis membranaceis, adpressis, calyce acutiusculo demidio brevioribus.

Dyckia rariflora, *Shultes fil.* in System. Veget. 7. 1195.

Description.—*Stem* short, stout. *Leaves* (7 inches long, 4-6 lines broad at the base) crowded, subulate, spreading, deflexed in the upper half, rigid; above, when young, channelled, afterwards flat, of uniform dark green colour, shining; below rounded, glaucous, striated; teeth aristato-spinous, distant, spreading, straight or hooked backwards or forwards, dark brown. *Flower-stalk* (2 feet high) elongated, erect, somewhat woolly at the top, scaly, the scales woolly, adpressed, and clasping, below resembling reduced suberect leaves, above less acuminated membranous and marcescent. *Flowers* (about 12) scattered loosely near the top of the stalk, without pedicels, arising in the axils of the scales, spreading, orange coloured. *Calyx* 3-parted, segments rigid, concave, ovate, overlapping at the base, adpressed, twice as long as the scales *Corolla* twice as long as the calyx, trifid, triquetrous when fully expanded, nectariferous at the base; segments slightly unequal in breadth, rhomboid, elongated at the base, spreading along the centre, compressed on the sides, somewhat undulate, united at the base with the calyx into a fleshy mass. *Stamens* 6, inserted into the corolla, but alternately in the centre of the segments and towards the edge so as to be opposite to the sepals, subexserted; filaments broad, flattened, slightly tapering at the apex; anthers bilocular, sagittato-oblong, bursting along the front, attached at the bottom of the sinus to the filament, and placed on the face of a broad connective; pollen granules minute, yellow, oblong. *Pistils* shorter than the stamens; style trifid, segments twisted together, and towards the apex each on its own axis, so as to give the linear stigma a spiral form. *Germen* oblong, 3-lobed, lobes cohering in the centre; ovules numerous, rounded, flattened from being crowded above each other, attached in two rows within each loculament to a central receptacle.

We received this plant from the liberal conductors of the Botanic Garden, Berlin, in 1832. It is very handsome, and flowered, for the first time, in the stove of the Botanic Garden, Edinburgh, in June 1835.

[*Additional Plants in next Number, the Descriptions having reached too late for insertion.*]

Proceedings of the Royal Society of Edinburgh.

1835, *2d February.*—Sir Thomas Makdougall Brisbane, K. C. B., President, in the Chair. The following communication was read:—

On the History of the Arch. By Dr Traill.

Dr Traill stated, as the result of a careful examination of the passages in ancient authors, supposed to prove the early use of arches by the Greeks, that we must abandon all hope of solving this disputed point, by a reference to those authors; and he instanced the danger of inferences from such uncertain data, by comparing the description of the Treasury of Minyas, given by Pausanias, with the still remaining kindred edifice, the Treasury of the Atridæ at Mycenæ. He also noticed the loose sense in which the ancient Greeks employed the terms ϑολος, ψαλις, ’αψις.

He next examined the evidence of the ancient use of arches, derived from existing monuments. He shewed the existence of true arched conduits in two very ancient Grecian edifices, the ruins of the Temple of Apollo Didymæus, in the territory of Miletus, and that of Apollo Delphineus at Athens; but he pointed out reasons why the Greeks never used the arch as a conspicuous member of their architecture, though these examples shew that they were not ignorant of the principle of the arch.

In reviewing the specimens of early Italian art, he shewed that the arch was well understood by the ancient Etruscans. He contended, from ancient authorities, and the peculiarity of the masonry, that in the Emissary of the Alban Lake, the Cloaca Maxima at Rome, the substructions of the Temple of Tarpeian Jove, and probably also in the body of the Pantheon itself, we have genuine specimens of very ancient Etruscan art, in which arches form conspicuous members; and he shewed that, in the walls of Cortona, Fesulæ, and Volterra, in the ruins of the Theatre of Arezzo, in the Piscina of Volterra, and in the sepulchres of Perugia and Tarquinii, we have undoubted and very ancient specimens of Etruscan arches. In the temples of Egypt we have no examples of any arch, one of the arches figured in Belzoni's tenth plate being evidently Saracenic, and the others a mere hole, with a rounded top, cut in a wall; but the author exhibited specimens of both round and pointed arches, lately delineated by Mr George Hoskins junior, in the ancient royal tombs of Napata and Meroe, which shew that arches were perfectly understood by the singular people of ancient Ethiopia.

The author next shewed that the pointed arch was long employed in eastern architecture before it was known in western Europe; a position which he illustrated by a collection of sketches from various authors.

He concluded this Essay by some observations on the extraordinary architecture of the ancient inhabitants of the table land of *Anahuac*, or Mexico, in which arches are distinctly visible; as may be seen in the designs of Dupaix, published in the magnificent work of Aglio; though the domes in the stupendous tombs of that people are constructed on the same principle as the Treasury of Atreus at Mycenæ.

16*th February*.—Dr HOPE, V. P. in the Chair. No paper read on account of extraordinary business.

2*d March*.—Sir THOMAS BRISBANE, K. C. B., President, in the Chair. A communication was read:—

On the Anatomy of the Char.

16*th March*.—The Right Hon. Lord GREENOCK, Vice-President, in the Chair. The following communications were read:—

1. The first part of a paper on the Ruins of Babylon. By Dr Traill.
2. On the application of the Hot Air Blast in the Manufacture of Cast-Iron. By Dr Clark, Aberdeen.

The author first gives a general account of the process of manufacture of cast-iron previous to the recent improvements, stating the quantities of the various materials put into the furnace, namely, of the Ore, the Fuel, and the Flux. He next states the method suggested first by Mr Neilson of Glasgow, and tried at the Clyde Iron-Works, for increasing the product of the furnaces with the same expense of materials, which consists in previously heating the air thrown into the furnace, in order to accelerate combustion. The method is found to produce a vast saving both in the fuel and the flux, although a certain portion of fuel has, of course, to be separately consumed for the purpose of heating the air, which is done by causing it to traverse a recurved pipe placed within a suitable furnace. During the first experiments, in 1830, the air was heated to 300° Fahrenheit. In 1831, Mr Dixon of the Calder Iron-Works thought of substituting raw coal for the coke which had hitherto been employed for fuel, at the same time that the air was still farther heated to 600° Fahrenheit, and with complete success. The result is, that *three times as much iron is now made by the use of a given weight of coal as formerly.* The following are the results of the experiments made at the Clyde Iron-Works.

In 1829, 111 tons of iron were produced from 403 of coke, or 888 of coal.
1830, 162 . . . . 376 . 886
1831, 245 554

Dr Clark endeavours to point out the source of advantage thus obtained, from the enormous quantity of air which is thrown into a furnace in full action, which is not less than six tons weight in a minute, and which, therefore, must exercise the most important influence upon the mean temperature of the furnace.

6th April.—Sir Thomas M. Brisbane, President, in the Chair. No paper read on account of extraordinary business.

20th April.—Sir Thomas Brisbane, President, in the Chair. The following Communications were read :—

1. The conclusion of Dr Traill's paper on the Ruins of Babylon.

The author, after noticing the ancient writers who have left us any description of Babylon, mentioned the short accounts of *Benjamin of Tudela*, of *Rauwolf*, *Pietro della Valle*, *Pere Emmanuel*, *Niëbuhr*, and *Beauchamp*, from which little information is to be derived, compared to what may be gathered from a succession of British travellers ; among whom, first in point of time and importance, is Claudius James Rich, late British consul at Bagdad, whose steps have been successively followed by Kinneir Ker, Porter, Buckingham, Keppel, and Mignan.

He next gave a general description of the ruins, and a more detailed one of the *Kasr*, the *Mujellibeh*, the *Embankment*, and the *Birs Nemroud.* The first he considered as the ruins of Nebuchadnezzar's palace; the second as belonging to the palace, or to its hanging gardens; the Embankments, with the hill of Amran, he believed to be a part of the defences of the city toward the river; and the Birs Nemroud, he considered with Rich as the ruins of the tower of Belus. He discussed at some length the arguments advanced by Rennell and Mignan against this latter opinion, chiefly founded on the account of Ctesias, that there were two palaces, one on each bank of the river, and that the principal one was on the *western* bank, and that the tower of Belus was on the opposite side. Ctesias being the only original writer who mentions two palaces, and his account not being confirmed by Herodotus, or any other ancient authority, and the glaring inconsistencies of his narrative in other respects entitling him to little credit, when he is not confirmed by other authors, it was concluded that we had no good reason to believe that two palaces had existed, far less that the most considerable was on the western bank of the river, especially as no remains, which can at all be regarded as the ruins of a palace, exist on that side; while in the Kasr we have remains of a pile, sumptuous in its material, of the finest furnace-baked brick, and magnificent for its extent, occupying a central position among the conspicuous ruins on the eastern bank, and enclosed on three sides by immense embankments, answering to the description left us by Herodotus of the strong interior wall which surrounded the palace.

The author next endeavoured to shew, that the Birs Nemroud answers better than any other of the remains to the description of the tower of Belus.

He next shewed, that the remains of reeds found between the courses of sun-dried bricks, correspond to Herodotus's description of reeds being used in the lower part of the Babylonian structures, —*δια τριῆκοντα δομων πλινθου*—" for thirty courses of brick," not as our translators have made it, " *between every thirtieth course.*"

Lastly, he examined the varying accounts of ancient authors with regard to the extent of the walls of ancient Babylon, and shewed, that if we adopt the least circuit given to them, that of Ctesias and Diodorus Siculus, it would make the circuit of the city 41 miles, while that of Herodotus would give a circumference of 55 miles,—either of them vastly greater than any idea we can form by comparison with the largest of modern cities: but the author observed, that there is every reason to believe that the vast area was not filled with houses, but contained fields and orchards interspersed; which is not only probable from what we know of modern Asiatic cities, but may be inferred from ancient authors, and especially from some remarks of Aristotle, who states that Babylon was rather " a community than a city, like the Peloponnesus " If we confine the city to the limits assigned by Diodorus, it will not include all the existing remains; whereas the limits assigned by Herodotus include all those venerable ruins in the vicinity of Hillah, that still astonish us by their stupendous dimensions.

2. On the Expansibility of different kinds of Stone. By Mr Alex. J. Adie, Civil Engineer.

This paper contains the results of an extensive series of experiments made upon different kinds of stone, as well as upon iron and upon brick, porcelain, and other artificial substances. The instrument employed was a pyrometer, of a simple construction, capable of determining quantities not greater than $\frac{1}{30,000}$ of an inch. The length of the substances generally employed was 23 inches. The general result of these experiments is, that the ordinary building materials of stone expand but very little differently from cast-iron, and that, consequently, the mixture of those materials in edifices is not injurious to their durability. The experiments from which the expansibility of the substances was numerically determined, were made between the limits of ordinary atmospheric temperature and that of 212°; steam being introduced for that purpose between the double casing of the instrument.

The following results were obtained for the fractional expansion of the length, for a change of temperature of 180° Fahr.:—

Table of the Expansion of Stone, &c.

	Decimal of length for 180° Fahr.
1. Roman Cement,	.0014349
2. Sicilian White Marble,	.00110411
3. Carrara Marble,	.0006539
4. Sandstone from the Liver Rock of Craigleith Quarry,	.0011743
5. Cast-iron from a rod cut from a bar cast 2 inches square,	.00114676
6. Cast-iron from a rod cast half an inch square,	.001102166
7. Slate from Penrhyn Quarry, Wales,	.0010376
8. Peterhead Red Granite,	.0008968
9. Arbroath Pavement,	.0008985
10. Caithness Pavement,	.0008947
11. Greenstone from Ratho,	.0008089
12. Aberdeen Gray Granite,	.00078943
13. Best Stock Brick,	.0005502
14. Fire Brick,	.0004928
15. Stalk of a Dutch Tobacco-pipe,	.0004573
16. Round rod of Wedgewood Ware (11 inches long),	.00045294
17. Black Marble from Galway, Ireland,	·00044519

27th April.—Sir Thomas M. Brisbane, President, in the Chair. The following communications were read:—

1. On the Action of Voltaic Electricity on Alcohol, Ether, and Aqueous Solutions. By Arthur Connell, Esq.

2. A Review of some of the more important Physical Truths contained in the writings of the Greek Philosophers, preparatory to an attempt to show that the more ancient language of Greece was based upon the Truths of Natural Philosophy. By the Rev. Archdeacon Williams.

Proceedings of the Wernerian Natural History Society.

1835, *March* 21.—Dr Charles Anderson, formerly, V. P. in the chair :—

A paper was read on the Geology of the Island of Skye, by R. J. Hay Cunninghame, Esq. The author described first, the stratified rocks in the ascending order, from the primary strata to the oolitic deposits occurring in the island ; then the unstratified or plutonic masses; and concluded with an enumeration of the relations of the two great series. Numerous sketches and sections and specimens illustrating his remarks were exhibited.

Sir Patrick Walker then laid before the meeting a series of fine marbles, which he had brought from the quarries of Bagneres de Bigorre.

Professor Jameson exhibited and described a series of bird from the Himmalayan Mountains, considered as identical, or nearly so, with the European: Strix passerina; Alcedo ispida; Parus major; Motacilla alba, boarula, Garrulus glandarius; Caryocatactes vulgaris; Ardea nyticorax; Numenius arquata, phœopus; Tringa squatarola, hypoleucos, pusilla, ochropus ; Charadius pluvialis; Cursorius hæmantopus; Podiceps minor; Œdicnemus crepitans ; Pterocles arenarius ; Anas clypeata, penelope, querquedula, crecca. After exhibiting and comparing the trivial characters of the above species with the European, it was stated, that even if all the external characters were the same, but if the shape of the head differed, we were entitled, from that character alone, to make a new species ; and as illustrative of this opinion, it was stated, on the authority of Brehm and others, that, generally speaking, no two species with plumage, &c. the same, but with different shapes of head, agreed in their habits and manners, nor were they ever found to breed with each other. Professor Jameson also exhibited a specimen of the female of the Cypselus longipennis, which he had received from Northern India, and stated that it only differed from the male figured by Temminck and Swainson, in wanting the brownish-red patches on the side of the neck ; in other characters it is identical.

1835, *April* 4.—Dr CHARLES ANDERSON, formerly V. P. in the chair.

There was read a communication from Mr R. H. Parnell regarding some new and rare fishes which he had procured from the Frith of Forth. In addition to the ample list of fishes found in the Forth, given by Dr Neill, and published in the Transactions of the Society, the author has detected nine others, two of which are new to science; one he referred to the genus Solea, the other to that of Platessa.

New Species of Sole.—This evidently enough belongs to the family Pleuronectes, but perhaps can scarcely find a place in any genus at present known. It varies much in shape, sometimes resembling a Brill, at others a Sole; in character it mostly resembles the genus Solea, in having the teeth deficient on the eye side, but in consequence of the mouth not being twisted, and the whole fish covered with a strong cuticle, it cannot be placed in that genus; it differs from all the other genera in having the teeth deficient on the eye side. When recent, length sixteen inches, breadth nine and a half, and one inch thick; mottled with white, yellow, and brown; the lateral line arched, two inches in length, and a quarter of an inch in breadth over the pectoral fin, from thence running straight to the tail. Jaws equal, each furnished with a row of obtuse cutting teeth, very closely set together, extending but half way round, and being deficient on the eye-side. The first two teeth on the lower jaw on the eye-side are the largest, and a little apart from the others; the eyes are large, situated on the right side, the irides of a light yellow colour; pectoral fins with nine rays; ventral fins five, anal fin seventy-three. Dorsal fin ninety rays, the first commencing over the eye, and running within half an inch of the tail; caudal fin rounded at the end, with sixteen rays. The whole fish is covered with a strong cuticle, rendering the scales very adherent, and the whole surface smooth. The first specimen which Mr Parnell observed was in February last, which he sent to Dr Greville, who made an accurate drawing of it. In March he found them more plentiful, and in April they entirely disappeared; they are known to the fishermen by the name of French Sole, and appear to be confined to the Fifeshire coast, where they are taken with the hook on the fine sandy banks.

New species of Platessa.—This fish seems much rarer than the last, as Mr Parnell has not had an opportunity of observing more than two specimens. It agrees with the genus Platessa, in having the mouth entire, with a row of obtuse cutting teeth round each jaw, tail rounded at the end, and the eyes placed on the right side. It differs from the Platessa vulgaris, in having no tubercles on the head; from the Platessa flesus, in not having a band of small spines on the side line; from the Platessa limanda, in not having the scales ciliated at their margin. It approaches nearer to the Platessa microcephalus, (as a variety of which it was regarded by Dr Neill); but it differs from it in having the lateral line nearly straight, the lower jaw longer than the upper, and the scales large. In shape it resembles the sole. Length sixteen inches and a half, breadth eight and a half, and one inch thick; lateral line arched one-eighth of an inch over the pectoral fin; eyes large, situated on the right side, irides silvery; mouth small, under jaw the longest, teeth small, closely set together; pectoral fin with eleven rays, ventral fins with six rays, anal fin with ninety-three rays; dorsal fin consists of 109 rays, the first ray commencing over the eye, and running within half an inch of the tail; the caudal fin rounded at the end, furnished with twenty-three rays. Scales large, very deciduous; the whole fish is of a yellowish-brown colour. It is known to the fishermen under the appellation of Craig Fluke, but they appear to confound it with the last, in consequence of some similarity.

Pleuronectes limandanus.—This fish is by no means rare in the Frith of Forth; it is known to most of the fishermen by the name of Sandnecker, or long fluke. Bloch has noticed it, under the name of Pleuronectes limandanus, as inhabiting the northern seas, but it has not as yet found its way into the works on British Ichthyology. Length ten inches, breadth four and a half, and much resembling the halibut in shape; lateral line nearly straight; mouth large, each jaw is furnished with a row of obtuse irregular sharp-pointed teeth, set a little apart from each other; pectoral fin consists of eight rays, anal fin of sixty rays; dorsal fin of eighty-five rays; the tail forms an angle, when expanded; it is furnished with sixteen rays; scales large, ciliated at their free extremity, which renders the fish rough to the touch; the whole fish is of a brown colour. They are taken with the hook in the sandy parts of the Frith of Forth, mostly on the

Fifeshire coast. The remaining species mentioned by Mr Parnell were, Gadus mustela, Pholis lævis, Blennius galerita, Merlucius vulgaris, Trigla cuculus, and Salmo albus. A series of specimens and drawings illustrating this paper were exhibited.

Professor Jameson exhibited and described a series of quadrupeds and birds. Among the more interesting of the quadrupeds were the Hylobates lar, leuciscus, albimanus, and hoolock; the latter of which, however, he stated, was probably not a true species, but the female of the Ounko of Frederick Cuvier.

Among the birds, two were described as new to science, viz. Aquila nigra, and male of Lophophorus Nigelli.

Aquila nigra.—Bill yellowish-brown, length 2 inches; length of gap 2¼ inches; cutting edge of upper mandible furnished with a protuberance. Nostrils ovoid. Face between the eyes covered with stiff hairs, which radiate as it were from a centre. Body, tail, and legs of a reddish-brown colour, with the exception of the middle of the back and rump which are greyish-white; length of body from tip of bill to tip of tail, 3 feet; from the tip of one wing to the tip of the other, 5 feet 7 inches; wings about 3 inches shorter than the tail. Tail square, but rather rounded, consisting of twelve feathers, the four centre ones being slightly banded below with greyish-white; length 14 inches. Legs feathered to the toes. Toes furnished with three scutellæ, which are largest on the back one; feet yellow; claws bluish-grey. *Hab.* South America.

The Professor applied the specific term Nigra to this bird, from black being the predominant colour of its plumage, and remarked with regard to the generic name Aquila, that it belongs to that genus, of which the type is Aquila fulva, from the cutting edge of the upper mandible being furnished with a protuberance; the wings considerably shorter than the tail; tarsi feathered to the toes; and lastly, the first phalanx of all the metatarsal bones being provided with three scutellæ.

Lophophorus Nigelli, male.—This bird was remarked by the Professor to differ from the female already described, in being larger, in having two reddish-brown bands, the one extending from the external angle of the eye, the other from the lower part of the auricular coverts, down to the under part of the neck, where they unite and form a broad diffused ring round it; in the breast being yellowish-white, and some of the feathers with a band of black in their centre; and lastly, in having the feathers of the hypochondriac region more strongly marked, and tipped with a much deeper brown. Like the female, it wants the spur. From the form of the bill, and the absence of the spur in the male, which is so prominent in the other species which have been included in the genus Lophophorus, this has now formed a new genus. The distribution of this species, the Professor remarked, was very wide, from its occurring from Persia, where the female was first discovered, onwards to the Himmalayan mountains.

A paper was read on the Manners and Customs of the Boshmen of the Sternberg Orange River, communicated by Mr Leslie, and a notice on the Deluges of Deucalion Ogyges, and Noah.

April 25.—Dr Robert K. Greville, V. P. in the Chair.—Mr James Wilson read an account of the new or rare insects, particularly Coleoptera, found by him during his late tour in Sutherlandshire. After some general observations on the distribution of insects in Scotland and England, and especially on the occurrence of some Scandinavian species in the north of Scotland, and of some of the species belonging to the warm climates of Europe, and in the south of England; the author enumerated and exhibited the principal species he had collected, and made remarks on such as were new, rare, or otherwise interesting.

The Secretary read an essay on the composition and qualities of a new building-concrete, communicated by Mr Stevenson. The paper was prefaced by a variety of historical details on the subject, which was illustrated by specimens.

The Secretary also read an account of a series of new and rare plants collected during an excursion, in the summer of 1834, to the United States and Canada, communicated by Mr James Macnab. This communication is printed in the present number of this Journal, (*supra*, p. 56, &c.)

Professor Jameson exhibited a series of new and rare birds; among the latter were the *Semi-palmated Goose, Charadrius nigrifrons; Haematopus ostralegus*, from New Holland; *Otis ruficollis*, South Africa; *Tantalus plumbeus*, South America, &c. He also described an Ibis, Tanagra, and Rubecola, new to science.

Ibis spinicollis.—Bill curved, and of a brownish-black colour; upper mandible furnished at its base with 13 greyish bands, each about 1½ line in length; length 7 inches, with the upper mandible, projecting over the other at the point. Nostrils linear, and inserted into a groove which extends along the bill to the tip, about ¾ of an inch from its base. Head destitute of feathers, also the centre and fore-part of the neck, to a distance of 1½ inch. Neck, fore-part covered with straw-coloured spines, on the back and upper part and sides with short greyish-white downy feathers, under with short bluish-black metallic feathers. Body, above of a brownish-black, each feather being alternately banded with dull and metallic reflections; below greyish-white. First and fourth feathers of wing longest, second and third equal; wings nearly as long as the tail. Length from tip of bill to tip of tail 3 feet 3 inches, from one extremity of the wing to the other 4½ feet. Tail square, and of a greyish-white colour, consisting of 12 feathers, length 7½ inches. Legs of a blood-red colour, feathered to about the middle of the tibia. Tarsus, length 4 inches.—*Hab.* Banks of the Murray River, interior of New Holland. From the slender bill, the head, and small part of the neck being destitute of feathers, this bird, the Professor remarked, formed a connecting link between the two divisions of the genus Ibis, Cuv. The specific term *spinicollis* was applied to it from the fore-part of the neck being covered with spines.

Tanagra nigricephala.—Bill bluish-black, conical, and much shorter than the head; length 5 lines, gap 7; upper mandible notched at the point, and slightly hooked. Nostrils circular and naked, inserted into the base of the bill. Head of a bluish-black colour; from the outer angle on both sides of the nostril a band of bluish-black extends across the temples to the root of the neck; from the inner, one of greyish-white extends across the ophthalmic region down to the nape. Throat white, with a bluish-black band on both sides, extending narrow from the base of lower mandible, and becoming very broad as it reaches the neck. Body, above of a saffron-yellow, approaching to siskin-green; below of a bright golden-yellow, mixed with orange. Wings of a bluish-black colour, with all the primary and secondary quills tipped with greyish-white, the first primary quill excepted; third quill-feather longest, second and fourth nearly equal, and longer than the first; upper wing-coverts bluish-black, mixed with greenish-yellow; under, yellowish-white. Tail greyish-black, and nearly square; length 3½ inches, and consisting of 9 feathers; upper wing-coverts greenish-yellow, under greyish-white; feathered to the tarsi. Tarsus, length 8 lines, and covered by 5 very broad scutellæ; middle toe 9 lines.—*Hab.* West India Islands. In the shortness and form of the bill, in the length of wings in proportion to the tail, in the arrangement of the quill-feathers of wing, the external toe united to middle by first joint, and the form and size of the scutellæ, this bird, the Professor remarked, seemed to hold a prominent place between the genus Pyrgita and Tanagra.

The Professor applied the specific term *Tytleri*, in honour of the late Lieutenant Tytler, a very active ornithologist, whose labours in India have added much to the interest of the Royal Museum of the University of Edinburgh, to a *Rubecola* which he described; and remarked, that although it agreed in the grouping of its colours with the common robin, yet, in the form of the bill, it presented as it were a link between the genus Rubecola and Phœnecura. Was sent to the Royal Museum by Lieutenant Tytler from the Himmalayan Mountains.

A series of specimens of the Muscipeta paradisi, Cuv., were exhibited by the Professor, for the purpose of pointing out that the Muscipeta indica is but a sexual variety; and he stated this from the examination of a large series of specimens lately received from the Himmaleh Mountains, some of which shewed the passage of the one into the other. In its distribution this species is very wide, occurring spread over all India and China; and identically the same species is found in Africa.

A fine live specimen of the Noctua nivea, hitherto confounded with the Noctua nyctea, it was announced had been found in Orkney.

At this meeting Dr Juan Llacayo, of Madrid, was chosen a foreign corresponding member.

Proceedings of the Society for the Encouragement of the Useful Arts in Scotland.

11*th March* 1835.—Mr Professor Forbes, V. P. in the Chair —The following communications were laid before the Society:

1. Description of a Plan by which the Study of Practical Che-

mistry may be rendered more accessible to all classes of Society. By Dr. D. B. Reid, Lecturer on Practical Chemistry, Edinburgh, and member of the Society of Arts.—The method of operating was illustrated by experiments.

2. On the best Composition for Rollers employed in applying the Ink to the Types in Letter-press Printing; especially with a view to preserve their adhesive and elastic properties in as uniform a condition as possible during damp and other variable states of the atmosphere. By Mr Robert Muir, of Muir, Gowans and Co. Printers, 42 Argyll Street, Glasgow.—A specimen roller for trial was exhibited.

3. Notice of a new Method of cutting and working Glass by the use of Turpentine. Communicated by Mr John Adie, 58 Prince's Street, Edinburgh.

4. DONATION.—On the Refraction and Polarization of Heat. By James D. Forbes, Esq. F. R. SS. L. & E. and F. G. S., Vice-President of the Society of Arts, &c. From the Transactions of the Royal Society of Edinburgh, vol. xiii. From the Author.

Mr Alexander Crichton, Lithographic Printer, 27 West Register Street, was admitted an Ordinary Member.

25th March.—WILLIAM MACDONALD, Esq. of Powderhall, V.P., in the chair.

1. Description of Paddles for Steam-Boats on a new plan. By Mr John Jaffray, Glasgow.—A model was exhibited.

2. Description and Drawing of a Machine for Sifting and Sizing Tea. Invented by Mr Wm. Darling, at Mr Ronaldson's, grocer, 6 St Patrick Square, Edinburgh.—A model was exhibited.

3. Description and Drawing of a New and Improved Method of Raising and Lowering the Venetian Blind, without fixing the cord at the various heights. By Mr John Weir, Venetian Blind-maker, 35 Gordon Street, Glasgow.—One of the blinds was exhibited.

4. Description and Drawing of Improved Pincers for Drawing Nails. By the same.—The pincers were exhibited.

5. Specimens of Drill and other Turning in Ivory and Metal were exhibited. By J. Graham Dalyell, Esq. advocate. Mr Dalyell described the process by which the effect in these specimens is produced.

April 8.—JAMES L'AMY, Esq. of Dunkenny, in the Chair.

1. Drawing and Description of a New Plan of a Paddle-Wheel for Steam Navigation. By William Dockar, Esq. Findon, by Banff. —A model in brass was exhibited.

2. Twelve Specimens of Lithographic Printing. By Mr Samuel Leith, Lithographer, Banff, Assoc. S. A. were exhibited.

3. A Drill Stock was exhibited, adapted for holding Drills made of Steel Wire of any diameter, within certain limits. By J. R. Edinburgh.

4. Specimens of very minute Engraving on Steel Punches, by Bourguing of Paris, were exhibited by John Robison, Esq. Sec. R.S.E. and M.S.A.

5. Specimens in metals, and other materials, of Effects which may be produced in the Turning-lathe, by regulating movements of the Tool and of the Object operated upon, were exhibited by the same.

April 22.—Mr Professor Forbes, V. P., in the chair.

1. On the Application of the Compressibility of Water to practical purposes. By James D. Forbes, Esq. F.R.SS. L. & E., Professor of Natural Philosophy in the University of Edinburgh, Vice-President of the Society of Arts.

2. Description of a Method by which Low-Pressure Steam was carried to the distance of 193 yards to work a Steam-Engine for draining a Coal-mine;—also a Method for stopping the Trams or Waggons from running down an Inclined Plane, if the Chain employed in dragging them up should break. By Mr D. Landale, Mining Engineer, Wemyss, Fife.

3. Remarks on the Glasgow and Garnkirk Railway, with illustrative Sketches. By Mr George Martin (at Messrs Grainger and Miller's, Civil Engineers), 108 George Street, Edinburgh.

4. Description of a New Clinometer, which serves also as a Portable Surveying Instrument, or Theodolite. Invented by Mr John Dunn, Optician, Hanover Street, Edinburgh, Curator Society of Arts.—The instrument was exhibited.

5. Donation.—Report to the Committee of the Commissioners of the Northern Lights, appointed to take into consideration the subject of Illuminating the Light-houses by means of Lenses. By Alan Stevenson, Esq. M.A. Civil Engineer. Edinburgh 1835. From the Author.

6. The following Candidates were admitted as Ordinary Members, viz.—J. Graham Dalyell, Esq. Advocate, Hanover Street; James Borthwick, Esq. Manager of the North British Insurance Company, 14 Claremont Street; Captain Robert Stuart, R.N., 2 Lynedoch Place; Mr William Bryden, Bell-Hanger, 82 Rose Street.

May 20. 1835.—Alexander Adie, Esq. in the Chair.

1. Additional Specimens of Impressions from Type Plates, prepared in a much shorter time, and at much less expense, than those commonly used for Magazines and other periodical publications having a very large circulation. By Mr L. Schonberg, Teacher of Drawing and Languages, Aberdeen.

2. Notices of Arrangements for Measuring the Angles of Crystals viewed under a high magnifying power. By the Rev. Edward Craig, A. M. Oxon, Counsellor, Soc. Arts.

3. Notice of a Walking Stick Umbrella, invented and used in France. Communicated by Robert Horsburgh, Esq. Treas. Soc. Arts. The umbrella was exhibited.

4. Notice of an Improved Form of Boots and Shoes, adapted to the comfort of the wearer. By Mr James Dowie, boot and shoemaker to the King, 57 Frederick Street, Edinburgh, M. S. A. Specimens were exhibited.

5 Donation.—Nautical and Hydraulic Experiments; with nu-

merous Scientific Miscellanies. By the late Colonel Mark Beaufoy, F. R. S. &c. Vol. I. London, May 1834. Presented by his son, Henry Beaufoy, Esq. F. R. S. South Lambeth, the publisher. Printed at Mr Beaufoy's own expense, for private distribution only.

6. DONATION.—On a valuable means of applying Friction and Heat in Chronic Inflammation and swelling of the Eyelids; with one of the Glass Phials employed for that purpose. Invented by the Right Hon. Sir John Sinclair, F. R. S. E, M. S. A. 25th March 1835. From the Author.

The following gentlemen were admitted Ordinary Members, viz.—Mr. Patrick S. Steele, merchant, 61 George Street; the Rev. D. T. K. Drummond, A. M. 9 Rutland Street, Edinburgh; Mr Francis Rankin, Glass Warehouse, 16 Picardy Place.

Henry Beaufoy, Esq. F. R. S. South Lambeth, London, was elected an Honorary Member.

List of Patents granted in Scotland from 25th September 1834 *to* 10*th June* 1835.

1834.

Sept. 25. To Matthew Bush, of Dalmonach Printfield, near Bonhill, by Dumbarton, in North Britain, calico printer, for an invention of "certain improvements in machinery or apparatus for printing calicoes and other fabrics."

Oct. 3. To Amasa Stone of Johnstone, in the county of Providence, and State of Rhode Island, in the United States of America, machinist, now residing at Liverpool, in the county of Lancaster, for an invention of "an improvement in power and other looms used in the weaving of silk, hempen, cotton, woollen and other cloth."

16. To Thomas Scarle of Coleman Street, in the city of London, merchant, in consequence of a communication made to him by a certain foreigner residing abroad, for an invention of "certain improvements in boilers for generating steam."

To Claude Marie Hilaire Molinard of Brewer Street, Golden Square, in the county of Middlesex, merchant, in consequence of a communication made to him by a certain foreigner residing abroad, for an invention of "a certain improvement in looms, or machinery for weaving fabrics."

To James Jamieson Cordes of Idol Lane, in the city of London, merchant, in consequence of a communication made to him by a late resident of the United States of America, now deceased, for an invention of "a certain improvement or improvements in machinery for making rivets and screw blanks or bolts."

To James Jamieson Cordes of Idol Lane, in the city of London, merchant, in consequence of a communication made to him by a late resident of the United States of America, now deceased, for an invention of "a certain improvement or improvements in machinery for making nails."

23. To James Watton of Sowerby Bridge, in the county of York, cloth-dresser, for an invention of "certain improvements in cards for carding wool, cotton, silk, and other fibrous substances."

24. To Jean Baptiste Mollerat, now residing with Sir John Byerley, at Whitehead's Grove, in the parish of St Luke, Chelsea, in the county of Middlesex, manufacturing chemist, for an invention of "certain improvements in the manufacture of gas for illumination."

Oct. 31. To Andrew Hall of Manchester, in the county of Lancaster, manufacturer, and John Slack the younger, of Chorlton-upon-Medlock, in the said county, putter out, for an invention of "improvements in the construction and working of looms for weaving by hand or power."

Nov. 14. To Charles Itherton of the city of Glasgow, engineer, for an invention of "an improvement or improvements on steam engines."

19. To Joseph Gibbs of Kennington, in the county of Surrey, engineer, for an invention of "certain improvements in wheels for carriages."

Dec. 12. To Alexander Craig of Edinburgh, in consequence of a communication made to him by a certain foreigner residing abroad, for an invention of "improvements in steam engines."

1835.

Jan. 5. To James Jones of Salford, in the county of Lancaster, machine maker, for an invention of "certain improvements for making rovings, spinning and doubling cotton, silk flax, and other fibrous substances."

7. To Samuel Garner of Lombard Street, in the city of London, gentleman, in consequence of a communication made to him by a certain foreigner residing abroad, for an invention of "an improvement in the art of multiplying certain drawings and engravings or impressions."

To George Dickinson of Buckland, near Dover, in the county of Kent, paper-maker, for an invention of "an improvement or improvements applicable to making of paper."

15. To James Couch of Stoke, Devonport, captain in the Royal Navy, for an invention of "certain improvements in ship's channels."

14. To John Johnson and George Johnson junior, hatters in Leith, in the county of Edinburgh, and James Johnson, Alexander Johnson, and Joseph Johnson, hatters in Edinburgh, in the aforesaid county, for an invention of "certain improvements in the manufacture of hats, caps, and bonnets by machinery, and for rendering the same waterproof."

23. To Samuel Seaward of the parish of All Saints Poplar, in the county of Middlesex, engineer, for an invention of "certain improvements in the construction of steam engines."

30. To William Wright, hatter, North Bridge, Edinburgh, for an invention of "a new and improved method of manufacturing hats."

Feb. 2. To Joseph Ferguson of Carlisle, manufacturer, for an invention of "a certain combination of processes whereby a new kind of dress or finish is given to certain goods."

3. To Henry Crosley of Hooper Square, Leman Street, in the city of London, civil engineer, for an invention of "an improved method or process, arrangement and combination of apparatus, with certain agents used or employed therewith, whereby evaporation of fluids and solutions may be effected advantageously, and also for other beneficial purposes to which the said method or process is applicable or can be applied."

6. To James Hudson of Gale, near Rochdale, in the county of Lancaster, calico-printer, for an invention of "certain machinery and apparatus applicable in block printing, or silk woollen cotton, and other fabrics, and on paper."

To Peter Fairbairn of Leeds, in the county of York, mechanist, for an invention of "an improved method of preparing, slivering, or roving hemp, flax, and other fibrous substances, for spinning."

10. To Alexander Shanks junior, flax-spinner in Arbroath, in the county of Forfar in North Britain, for an invention of "certain improvements in machinery for preparing and dressing hemp and other fibrous substances."

12. To James Kay of Pendleton, in the county of Lancaster, flax-spinner, for an invention of "a heckling machine of a new construction."

March 5. To Joseph Jones of Oldham, in the county of Lancaster, cotton manufacturer, and Thomas Mellodew of the same place, mechanic, for an invention of "improvements in looms to be worked either by hand or power, for weaving of plain, corded, or figured fabrics, and an apparatus for better effecting the cutting of fustians or other fabrics woven in a diagonal form."

10. To William Aitken of Aberdeen, in the county of Aberdeen, North Britain, Esquire, for an invention of "certain improvements in the construction of carriages to be propelled by animal or other power."

To Thomas Alcock of the parish of Claines, in the county of Worcester, lace-manufacturer, and John Bertie of Basford, in the county of Nottingham, machinist, for an invention of "an improved texture of the lace hitherto called bobbin-net or twist net, and also certain improvements in lace machinery for producing ordinary bobbin-net, and for producing ornaments therein."

16. To William Hale of Colchester, in the county of Essex, civil engineer, for an invention of "certain improvements on, or additions to, boilers or apparatus for producing motive power."

To Daniel Duff of Dundee, in the county of Forfar in Scotland, for an invention of "a method of softening, splitting, and preparing hemp and other fibrous substances by machinery, for the purpose of being spun into yarns."

23. To William Hale of Colchester, in the county of Essex, civil engineer, for an invention of "certain improvements in or on windmills, which improvements are applicable to other purposes."

To Philip Augustus de Chapeaurouge of Fenchurch Street, in the city of London, gentleman, in consequence of a communication made to him by a certain foreigner residing abroad, for an invention of "a machine, engine, or apparatus for producing motive power, which he denominates a self-acting motive power, and is called in France by the inventor 'volant moteur perpetuel."

25. To Charles de Bergue of Clapham, in the county of Surrey, gentleman, for an invention of "certain improvements in machinery for spinning or twisting cotton, flax, silk, and other fibrous substances."

27. To Robert Joseph Barlow of Rudley, in the North Riding of Yorkshire, clerk, for an invention of "certain improvements in springs, applicable to carriages and other purposes."

To John Sylvester of Great Russell Street, in the county of Middlesex, civil engineer, for an invention of "improvements in apparatus used in the communication or transmission of heat to aeriform, liquid, and solid bodies."

To Henry William Nunn of Whippingham in the Isle of Wight, George Mowbray, and Richard Alabone, both of the town of Newport in the said island, lace-manufacturers, for an invention of "improvements in manufacturing certain kinds of embroidered or ornamented lace."

31. To Samuel Draper, late of Radford, in the county of Notts, but now of Basford, in the same county, lace-maker, for an invention of "an improved machine for making an improved manufacture of figured bobbin-nett, or what is commonly called bobbin-net lace."

April 3. To William Newton of the Office for Patents, Chancery Lane, in the county of Middlesex, civil engineer, in consequence of a communication from a foreigner residing abroad, relating to an invention of "certain improvements in preparing fibrous or textile plants, either indigenous or exotic, to be used in place of flax or hemp."

6. To James Hunter of Leys Mill, Arbroath, in the county of Forfar, North Britain, mechanic, for an invention of "certain improvements in the art of cutting, or what is commonly called facing and dressing certain kinds of stone."

10. To John Day of York Terrace, Peckham, in the county of Surrey.

gentleman, for an invention of "an improvement or improvements in the construction of railways."

April 16. To James Stevenson of Leith, merchant, and John Ruthven of Edinburgh, mechanician, for an invention of "a method of cutting wood by certain improved instruments."

21. To John Somerville, clerk, minister of Currie, in the county of Edinburgh, for an invention of "certain improvements in the construction of guns or muskets and other such fire-arms."

22. To William Bruce, baker, in the city of Edinburgh, in consequence of a communication made to him by a person resident abroad, for an invention of "improvements in machinery or apparatus for making ship and other biscuit or bread."

May 12. To William Morgan of the Kent Road, in the county of Surrey, Esquire, for an invention of "certain improvements in steam-engines."

To James Aldous of Clapton, in the county of Middlesex, smith, for an invention of "certain improvements on steam-engines."

18. To James Slater of Salford, in the county of Lancaster, bleacher, for an invention of "certain improvements in addition to certain improved machinery for bleaching linen and cotton goods."

To Alexis Dumoulin of Leicester Square, in the county of Middlesex, merchant, for an invention of "certain improvements in gas apparatus."

To Moses Poole of the Patent Office, Lincoln's Inn, in the county of Middlesex, gentleman, in consequence of a communication made to him by a certain foreigner residing abroad, for an invention of "certain improvements on trusses or instruments for the cure of hernia or rupture."

To Robert Whiteside of Ayr, in the county of Ayr, wine-merchant, for an invention of "certain improvements in the wheels of steam-carriages, and in the machinery for propelling the same, also applicable to other purposes."

To John Buchanan of Ramsbottom, in the county of Lancaster, millwright, for an invention of "certain improvements in the construction of cylinder printing machines, used for printing paper, calico, and fabrics."

To William Simpson Potter of Verulam Buildings, in the county of Middlesex, ex-merchant, in consequence of a communication made to him by a certain foreigner residing abroad, for an invention of "improvements in rendering fabrics waterproof."

To James Boydell of Dee Cottage, in the county of Flint, land-agent and surveyor, for an invention of "an improvement in machinery or apparatus for moving or towing boats or other vessels."

19. To Francis Humphrys of York Road, in the borough of Lambeth, in the county of Surrey, civil engineer, for an invention of "certain improvements in marine steam-engines, which improvements are also applicable to steam-engines for other purposes."

29. To Samuel Slocum of the New Road, St Pancras, in the county of Middlesex, engineer, for an invention of "a certain improvement or improvements in machinery for making nails."

June 5. To Joshua Taylor Beale, of No. 11 Church Lane, Whitechapel, in the county of Middlesex, engineer, for an invention of "a lamp applicable to the burning of substances not hitherto usually burned in such vessels or apparatus."

10. To William Crofts, of New Radford, in the county of Nottingham, machine-maker, for an invention of "certain improvements in machinery for making bobbin-net lace for the purpose of producing, by the aid of such improvements, ornamented net or lace of various kinds."

To Charles Schafhautl of 77 Cannon Street, in the city of London, gentleman, for an invention of "an improvement in the mode of manufacturing malleable iron."

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

On the Volcanic Strata exposed by a Section made on the site of the New Thermal Spring discovered near the Town of Torre del Annunziata, in the Bay of Naples; with some Remarks on the Gases evolved by this and other Springs, connected with the Volcanos of Campania. By CHARLES DAUBENY, F. R. S. & F. G. S. Professor of Chemistry, Oxford.* Communicated by the Author.

THE discovery of a warm acidulous spring, possessing, as it is said, considerable medicinal virtues, on a point of land which runs into the sea near the town of Torre del Annunziata, at the foot of Vesuvius, has led to the removal of a considerable portion of the overhanging cliff, for the purpose of obtaining space enough for the erection of a pump-room, baths, &c.

A clear and distinct section has thus been exposed of the volcanic strata constituting that part of the slope of Vesuvius, of which a short account may perhaps be acceptable, as supplying evidence of two at least, if not of three, distinct eruptions of volcanic matter having overspread that country, within a period of history, the limits of which may be defined by the evidence afforded.

The entire height of the cliff, to which the accompanying drawing refers, taken from a point immediately behind the pump-room or bath-house, is 68 feet; of which from five to ten feet on the top consist of vegetable mould intermixed with decomposing lava.

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* Read before the Geological Society of London, 1835.

Immediately underneath this is a bed of lava, averaging five feet in thickness, hard and compact internally, but scoriform at bottom. It is throughout more or less cellular, and contains here and there some considerable cavities.

I am indebted to Colonel Robinson, an English officer of great enterprise and intelligence, at present in the Neapolitan service, for the knowledge of a fact relating to this lava, which seems of some theoretical importance. He assures me, that, in one of the cavities alluded to, he discovered a large quantity of a white pulverulent earthy matter, which, upon examination, turned out to be carbonate of magnesia. I regret that, as Colonel Robinson had parted with the whole quantity which he collected, it is out of my power to give to his statement all the authenticity which the exhibition of the substance itself would afford; but I may offer this corroboration of it, namely, my having myself found a white superficial coating on one part of this same lava, which certainly did appear to consist of, or at least to contain, a very large proportion of the earth in question combined with carbonic acid. Colonel Robinson also wrote me word, in a letter I received from him just before I quitted Naples, "that the large quantity of magnesia ascertained by the analysis, induced him to endeavour to find out its origin, and, for that purpose, he excavated to the depth of forty feet two miles up the mountain, in a direction with the spring, and found large pieces of pumice, of a brownish-red colour, very much resembling an honeycomb, the cavities of which were completely filled up with a perfectly white powder, which proved on examination to be carbonate of magnesia."

Now the occurrence of magnesia in such situations would seem to imply the fact of its sublimation, and to concede the possibility of this being one step at least towards the admission of Von Buch's much contested theory respecting the origin of dolomite.

Underneath the lava bed just mentioned, the cliff is principally made up of a succession of beds of rapilli and scoriæ, sometimes agglutinated through the medium of volcanic sand, at other times heaped together confusedly without any cementing material.

The beds themselves are of various shades of colour, grey, red, and black, according to the predominant tint in their component parts.

In the lower portion of the cliff these beds are for the most part tolerably distinct; but in the upper they are so blended together, as scarcely to be distinguishable one from the other. In the midst of these strata of rapilli occurs a bed of grey tuff, more compact than that covering Herculaneum, and in parts nearly as much so as the generality of that of Posilippo. It is of irregular thickness, swelling out in one place, and contracting in another. Its greatest thickness is about five feet. It is seen to terminate abruptly on both sides; on the east a little beyond the New Pump-room; on the west immediately behind that building. In the latter place it is interrupted apparently by a heap of large blocks of volcanic matter, which are seen abutting against its side. About ten feet farther to the west, however, and precisely on the same level, we observe the commencement of a narrow thread of the same species of tuff as before, which pursues a similarly horizontal course along the cliff to the west, gradually thickening until it acquires nearly an equal bulk to that of the bed of tuff first noticed. Hence it would seem like a continuation of the former bed, which had been broken off by the heaps of detached fragments of lava before noticed.

This second bed of tuff, however, after pursuing a tolerably regular course for about thirty feet along the cliff, breaks off as suddenly, and as abruptly as the former, so that both appear to be in a manner inclosed within, and surrounded by, the strata of rapilli. At a still lower level are one or two beds of tuff similarly constituted, though less distinct than the two already described, and these, in like manner, after continuing in an horizontal direction for a few feet, either break off abruptly, or are gradually lost by intermixture with the surrounding beds of rapilli. That these tufaceous strata owe their superior compactness only to the weight of the beds of loose rapilli superincumbent, and have been formed *on dry land* by the mere action of rains or torrents upon volcanic materials, appears from the occurrence, in a stratum of rapilli and volcanic sand eight or nine feet lower down the cliff, of an admixture of vegetable mould containing stems of reeds similar to those now growing in the neighbourhood.

About a foot still lower are likewise to be seen the roots, and part of the trunk, of a fir of considerable size, standing quite upright in the soil, on the very spot in which it must have grown.

Intermixed with the earth found at this level, have been discovered, as I understand, fragments of tiles, a piece of hewn timber, in the possession of Mr John Auldjo, and other traces of human art.

The latter gentleman, I believe, is also in possession of a part of a cypress, which, in an earlier stage of the operations, was discovered standing upright in the soil, at a somewhat higher level, with the outer part carbonized, but the interior perfectly sound. The circumference of this tree was seven feet, and the part left standing by the workmen was four feet in height. Subsequent operations have caused its removal.

Here, on driving an horizontal gallery into the rock at a level nearly ten feet below that of the first mentioned tree, the workmen lately discovered vestiges of walls and buildings, together with fragments of Roman pottery, traces of fresco paintings, and a considerable quantity of cut marble.

The excavations are still continued, under the superintendance of Colonel Robinson, and a report of the discoveries will no doubt be made in due time by competent persons. At present I can only remark, that sufficient has been explored to substantiate the existence, on this spot, of the remains, if not of another town, at least of several buildings that had been overwhelmed by one of the eruptions of Vesuvius.

What the date of the catastrophe itself may have been, it seems not difficult to determine. From their contiguity to Pompeii, which lay little more than two miles distant, the houses would almost inevitably have been covered by the same shower of volcanic materials, if they had been in existence at that time; and, as they are evidently Roman, no earlier date than this can be assigned to their interment, as we know that Vesuvius slumbered during the whole period of Roman history till the reign of Titus.

It has been conjectured that the houses discovered may be the remains of a place called Oplonti, mentioned in the Tabula Theodosiana, as situated between Herculaneum and Pompeii, six Roman miles from the former, and three from the latter, which agrees pretty well with the present site of Torre del Annunciata.

Others, on the contrary, are more inclined to suppose, that, inasmuch as in the course of the excavations a well was discovered of the same kind of water as that of the new mineral spring, which appears, from the pillars that support it, to have been constructed by the Romans, the buildings disinterred so immediately in its neighbourhood may have been erected for the accommodation of persons who resorted to it for its medicinal virtues.

These two opinions, however, may perhaps be reconciled by reference to the ancient manuscript referred to under the name of the Tabula Theodosiana. It will there be found, that to the name Oplonti is attached the drawing of a quadrangular building, and that on other parts of the same map, where a similarly shaped edifice is delineated, a thermal spring (Thermæ) is implied to have existed on the site. Oplonti, therefore, whatever may have been the size and character of the buildings of which it consisted (and of this the excavations now proceeding will ere long inform us), appears to have been connected with the thermal spring which existed there in the time of the Romans, and which has been rediscovered recently.

It is the date, however, of the eruption, and not the character or designation of the buildings overwhelmed by it, which interests us as geologists; and, respecting the former point, little hesitation, I conceive, need be entertained, in the identification of it with the great eruption of 79, which destroyed so many important cities in Campania.

Nevertheless, although the lower strata exposed by the section of the cliff were in all probability formed at the period just mentioned, the same could scarcely have been the case with those superincumbent.

The occurrence of a pine at the height of twenty-five feet, of stems of reeds at twenty-seven, and, according to Mr Auldjo, of a cypress at twenty-eight feet above the level of the sea (the pine and cypress standing evidently *in situ*), seems to shew, that this spot must have been covered over again by some later eruption, though whether by the same which produced the bed of lava, capping the summit of the cliff, we have no data for determining.

A discovery has, however, been lately made, which appears to define, within certain limits, the date of the latter event.

Near Bosco-tre-Case, a village which stands about two miles to the north of Torre del Annunziata, on the slope of Vesuvius, was found in the course of last summer, only a few feet beneath the soil, a bag of Roman gold coin, evidently almost fresh from the mint, and bearing the date of A. D. 572, with the impression of a Cross.

One of these coins I have myself seen in the possession of Colonel Robinson, to whom I am indebted for a knowledge of the fact itself, and the date inscribed upon it would seem to indicate, that it had been buried for concealment's sake by some inhabitant of that province during the alarm excited by the Lombard invasion. Perhaps, therefore, the second catastrophe may be attributed to the eruption of Vesuvius in the year 472, which is said by Procopius to have covered all Europe with ashes, and to have spread alarm even at Constantinople, and which, making due allowance for the exaggeration of that historian, seems to have been little inferior, in point of magnitude, to the celebrated one in the reign of Titus.

It is remarkable, however, that an event which caused the neighbourhood of Torre del Annunziata to be covered over to the depth of more than thirty feet, should not have added sensibly to the accumulation of volcanic materials about Pompeii.

The thermal spring which has lately been discovered on the spot alluded to, in consequence of borings made originally by Colonel Robinson, in the hope of supplying the town of Torre del Annunziata with drinkable water, possesses a temperature of about 87 degrees of Fahrenheit. It was analyzed by Professor Ricci of Naples, and appears from his account to abound chiefly in the bicarbonates of soda, magnesia, potass, and lime, together with sulphate and muriate of potass, muriate of soda, and muriate of magnesia.

It is chiefly remarkable for the large quantity of carbonic acid gas which rises up through the same aperture. The volume of this is sufficient, to maintain the water itself in a state of constant and violent ebullition, and to impregnate the air of the stone cylinder through which it escapes, to such a degree, as to render it absolutely unrespirable.

The carbonic acid has also been long observed to rise in bubbles through the sea-water near this spot, and I am assured by Colonel Robinson, that there are patches of land upon the cliff, upon which, owing to the disengagement of this same noxious gas, no plants can be made to grow. A similar statement with regard to other places within the precincts of Vesuvius, has been given long ago by Breislac.

The disengagement of carbonic acid throughout this district is indeed so well known to all who have visited Naples, and is so obviously referable to the action of heat upon the contiguous beds of limestone, that it does not seem to require in itself any particular notice.

It has not, however, I believe, been so generally remarked, that, in almost every instance, there is a slight admixture with it both of oxygen and nitrogen gases, in which, however, the former never, according to my experience, rises to the proportion present in atmospheric air.

This observation I have had the means of substantiating, not only in the precincts of Vesuvius, but also at the Lago di Amsanto amongst the Apennines, near Mount Vulture in Apulia, and at the Lago di Solfatara near Rome.

At the spring of Torre del Annunziata, after carefully removing the carbonic acid, I found the residuary gas to consist of oxygen 16, nitrogen 84; at the spring of Santa Lucia in Naples, of oxygen 14.5, nitrogen 85.5; at the Lago di Amsanto, of oxygen 9, nitrogen 91; at the Aqua Santa on Mount Vultur, oxygen 10, nitrogen 90; at the Lago di Solfatara, near Tivoli, of oxygen 9.5, nitrogen 90.5. I have not chosen to class with the above the mineral water of Castellamare, in the Bay of Naples, as in it the relative proportions of the carbonic acid, and the residuary gas disengaged, approached so much more nearly to an equality; that collected by me consisting of carbonic acid 53.0, nitrogen 47.0, oxygen 0.5.

With respect to the cause of nitrogen in springs, I do not in-tend to renew on the present occasion the discussion that has been carried on in the Philosophical Transactions between Dr John Davy and myself relative to that subject; a discussion, however, which, on whatever side the advantage may be considered to rest, I shall not regret having provoked, if it has had the effect of bringing the subject of the gases disengaged from thermal waters and from volcanos more prominently forward.

Without, however, entering into any further dispute with respect to the manner in which the evolution of air, from the site of the volcanic island off the coast of Sicily, may have been brought about, I will just remark, that the constitution of that which was evolved is such as to imply the previous abstraction from it of a part of its oxygen. Now, if the same fact can be established in almost every case in which air is evolved from a source, the connection of such phenomena with some process of oxidation seems fairly established.

To refer this abstraction of oxygen in general to animal or vegetable putrefaction is plainly impossible; and hence most persons will be led to seek for some processes or other connected with, or analogous to, combustion, as the most likely of any with which we are acquainted to have occasioned it.

And, if this be conceded, I am quite ready to wave all farther demands upon the acquiescence of my readers; for, with respect to the particular hypothesis which I have myself advocated, in which the basis of the earths and alkalies are assumed to be the substances thus oxidated, I only claim for it the merit of explaining the phenomena, which it appears to me cannot be shewn to belong to the rival theory embraced by Cordier.

Whilst, however, I still object to the mode in which Dr Davy conceives the nitrogen emitted round the volcanic island to have been deprived of so large a proportion of its accompanying oxygen, inasmuch as (waving other considerations) the same explanation could not be extended to the other analogous cases, I am less indisposed to admit that the gas may in many instances have been separated from the water, and not derived from an independent source.

Perhaps the gas evolved in the case stated by Dr Davy presents the greatest difficulties to such a solution, for it does not seem clear why the water, which had disengaged a portion of its air only in consequence of being heated, did not recombine with it upon becoming again cool.

The expulsion of air from water in a retort by ebullition does not seem a strictly parallel case, because the whole body of the liquid is here kept at an high temperature; whereas, we know that the upper strata, at least, of the sea-water, remained in the neighbourhood of the volcano at the same point of heat as in other parts of the Mediterranean. But, in the case of a thermal spring, where the whole body of the liquid has its temperature raised considerably, there is no difficulty in conceiving that its gas may have been expelled, and that it may pass upwards through the several strata of the fluid, until it reaches the air, without meeting with any capable of dissolving it.

Even the large quantity of gas disengaged at Bath may perhaps be attributed to the source itself, without resorting to any such theory as that by which, in my paper in the Philosophical Transactions in 1834, I was led to account for it, although in this case the carbonic acid with which the water is found impregnated, would have assisted in promoting its expulsion.

To those, however, who refuse to admit volcanic action to be a process of oxygenation, such a mode of explaining the emission of nitrogen would seem to remove the difficulty only one step farther, since it still remains to be shewn, why spring-water, which is in general impregnated more fully with oxygen than with nitrogen gas, should in these cases disengage chiefly the latter.

I ought also in candour to acknowledge, that, in none of the warm springs in the neighbourhood of Naples (for that of Castellamare is scarcely thermal), did nitrogen appear to be evolved in any such notable quantity, as happens amongst the extinct volcanos of Auvergne, in the thermal springs at the base of the Alps, and in other localities less obviously connected with volcanic agency.

The warm springs we meet with between Puzzuoli and Naples occur in wells which afford no opportunity for examination, but the mineral ones in Ischia, all of which I have visited, seem totally destitute, not only of this, but of every other gaseous impregnation.

Whilst admitting, therefore, that the hot springs of Campania do not furnish direct confirmation of the connexion between volcanic processes and the evolution of nitrogen, I contend, on the other hand, that they suggest no facts, which can set aside the evidence in favour of that position, which the production of ammonia within the volcano itself appears to furnish *.

It seems to me also, that an examination into the thermal springs of a volcanic district, with reference to this point, namely, as to whether they do or do not afford a channel for any gaseous emanations, may perhaps be a means of determining, either that the igneous action is at the time going on underneath, or that it has been transferred to some other quarter.

The former I take to be the case at Castellamare, at Torre, at the Solfatara ; the latter, I should conclude, from the absence of gaseous exhalations, with respect to Ischia, notwithstanding the continuance of a high degree of heat pervading the rocks of that island, which is evidenced by the abundance of thermal waters, and by other phenomena,—a heat imparted to them probably at a period as remote as that of the latest eruption which has occurred in that island, that, namely, of 1502.

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* See my paper in the Philosophical Transactions for 1835 on the late eruption of Vesuvius.

SECTION of the Cliff behind the New Pump-Room at Torre del Annunziata.

	68 feet. Summit of the cliff.
Vegetable Mould.	
Decomposed Lava, mixed with Mould.	63.
Bed of Lava, compact at top, scoriform at bottom.	58.
Beds of Rapilli, Blocks of Lava, and occasionally of Pumice, of various colours,—red, black, grey,—intermixed with, and sometimes slightly cemented together by, Volcanic Sand.	38.
Bed of Grey Tuff, of irregular thickness, from 1 to 5 feet.	34.
Beds of Rapilli and other volcanic materials.	28. { Level of the cypress marked in Mr Auldjo's drawing. 27. Stems of roots, &c. 25. Root and trunk of a fir. 22. Level of the houses discovered, according to the statement given in the plan.
Bed of red Rapilli, 2½ feet thick.	15. The first excavation made is in a stratum which sinks to this level at the spot from which this section is taken.
Bed of Grey Rapilli.	
Bed of Black Rapilli, loosely agglutinated.	
Rapilli heaped together, but not agglutinated.	
Rapilli loosely agglutinated, 2½ feet.	
Rapilli, not agglutinated.	
Dark coloured and loosely agglutinated Rapilli.	Level of the floor of the bath-house.

Inquiry into the Changes which the Atmosphere undergoes when in contact with certain Vegetables which are destitute of green leaves, &c. By M. F. Marcet.*

It is now a long while since, thanks to the labours of many physiologists, and, in particular, to those of our countryman M. Theodore de Saussure, we have been familiar with the action of the atmosphere in the nutrition of vegetables, which are possessed of leaves and other green parts. It is known that this action consists of many distinct operations, some of which tend to vitiate the atmosphere, either by depriving it of its oxygen, or by the formation of carbonic acid gas, whilst others, on the contrary, tend to purify it by the exhalation of a considerable quantity of oxygen gas. Theory, supported by experiment, goes to demonstrate that the latter of these processes exceeds the former, and that growing vegetables have a tendency to increase the quantity of the oxygen of the atmosphere. Thus they afford a kind of compensation for the continual absorption of oxygen, which goes on in combustion, and in the respiration of animals.

But there is a numerous class of vegetables, viz. that of cellular plants, which is completely devoid of green parts, and whose mode of nutrition appears to be very different from that of other plants; of which series mushrooms form the most important example; and every thing induces us to think that these plants are not like green ones, endowed with the property of decomposing carbonic acid gas through the agency of the light, since many of them thrive almost in complete darkness. How, then, is it that they assimilate to themselves the carbon which enters into their composition? and how is the work of nutrition effected generally in this class of vegetables? These are points of which we are almost in complete ignorance; and, therefore, I have thought that some experiments regarding the action of mushrooms upon the atmosphere, during the process of vegetation, was likely to throw some light upon the manner in which these singular vegetables are nourished.

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* From a Memoir read to the *Société de Physique et d'Histoire Naturelle de Genève,* on the 18th December 1834. Taken from the Memoirs of the Society, tom. vii. part 1st.

I had occasion, in 1827, to study the effects which mushrooms produced on water when placed in that liquid. The conclusions at which I then arrived, and which in a great measure coincided with the experiments conducted at the same period by M. Th. de Saussure, (communicated by him to this Society, but never published), appeared in the 40th volume of the "*Annales de Chimie.*" They go to show that mushrooms placed under water, in all cases disengage a gas composed of hydrogen and nitrogen, the proportions of which vary according to the period of examination. Some individuals have appeared to think that the disengagement of the gas was nothing more than the commencement of decomposition in the vegetable; but the fact that the one (Sphœria digitata) which I found to disengage the most is peculiarly tough in its nature, and consequently little liable to decomposition, especially at the end of a few hours, appears to me to meet and overturn this objection. Besides the difference in the quantity of the disengaged gas, according to the quantity of light to which the mushrooms were exposed, appears an additional reason for attributing the evolution of hydrogen and nitrogen to a vital action, and not to the decomposition of the vegetable.

It was only during the last summer that I resumed the investigation, the first part of which was communicated some years ago to the Society. In the interval, I had always hoped that the examination of the point would be again undertaken by my learned colleague, whom I have already named, and who would certainly have arrived at much more precise and satisfactory results than those which I have obtained. At the same time, the long interval of seven years having elapsed, there seems no ground to hope that he will farther prosecute it, and I have, therefore, thought it right to delay the investigation no longer. I have entered into this explanation, for the sake of reminding the Society that M. de Saussure had engaged in the investigation at the same time with myself; and so it may happen, that among the experiments which I am about to detail, there possibly may be some which have been already made by him, but the recollection of which has escaped me, since his unpublished memoir was read some years ago to the Society.

Desirous to learn, if I could, by a direct experiment, and without removing the mushroom from the spot where it grew, what were the changes which it produced upon the atmosphere during its growth, I at first attempted to subject it to experiment without removing it from the soil. With this intention, having covered with a large glass vessel, a plant which was just projecting from the earth, I surrounded its base with a kind of fatty lute, to which I made the bell-glass adhere, taking at the same time all possible precautions, to prevent all communication between the air in the receiver and the external atmosphere. After the lapse of two or three days, when the mushroom had greatly increased in bulk, the air under the bell-glass was subjected to analysis. This experiment was repeated many times, and always with the same result, viz. that in no instance had the confined air appeared to have undergone any sensible change, with the exception of the presence, from time to time, of an extremely minute quantity of carbonic acid gas.

The negative result of these experiments giving ground to fear that the apparatus was imperfect, and that it would be difficult, if not perhaps impossible, to prevent all communication, even that which might take place through the soil, between the external and included air, I found myself compelled to resort to another method, which is perhaps less direct, viz. to confine my examination to the atmospherical changes produced by mushrooms which have been raised from the soil, and which, of course, were not wholly in their natural condition. This plan, which has been successfully employed in the examination of the change in the atmosphere produced by green plants, is not quite free from objection, especially when we are working with vegetables so subject to decomposition, as are a great number of the mushroom tribe, and still more so, as it is often difficult to perceive the exact moment when they cease to live, and when the process of fermentation or spontaneous decomposition begins. To free myself as much as possible from this source of error, I took the following precautions. 1*st*, I took care to choose for my experiments those mushrooms only which were most consistent and tough, and the very nature of which must therefore have prevented the effects of a speedy fermentation. 2*d*, The experiment never lasted longer than a few hours, usually from eight to twelve; and I was always particular in remarking, at the end of every experiment, if the mushrooms which had been employed exhibited, by their smell or otherwise, the slightest appearance of decomposition; and if this were the case, that experiment was not regarded. 3*d*, I took care in removing the mushrooms from the soil, never to cut the tendrils of the root, and also to preserve round the stalk a very small quantity of mould, in the hope of prolonging vegetation as long as possible.* When I was experimenting on those kinds which grow on wood, I always removed a piece of wood with the mushroom, in fact the bed in which it grew, so that in this case the experiments were conducted without in the slightest degree removing them from their natural condition.

I now proceed to give an account of the results obtained, by successively introducing different kinds of mushrooms: 1*st*, into atmospheric air; 2*d*, into oxygen; and, 3*d*, into nitrogen.

SECT. I. *On the Action of Mushrooms on Atmospheric Air.*

Before introducing the mushrooms under the bell-glasses filled with atmospheric air, I carefully noted the weight and the number of the individual plants which I meant to submit to experiment. I then introduced them into a receiver, of a longish form, about three-fourths of which was filled with air. This receiver, which was graduated, was then placed in the mercurial trough. After allowing the mushrooms to remain a certain number of hours, and after having noted the change induced in the volume of the air in the receiver, I submitted this air to analysis, by means of Volta's eudiometer. Now, then, for the experiments.

1*st Experiment.*—Three mushrooms, of the species *Lycoperdon bovista* (the fuzz-ball), weighing together 130 grains, were introduced under a receiver containing about 32.37 cubic feet of air, at the temperature of 65° Fahr. Here they remained, fully exposed to the light, but not in the direct rays of the sun, for nine hours. At the expiration of this time, the volume of the air in the receiver was 32.95 cubic feet. The analysis furnished the following result:—The receiver contained, before the experiment, of nitrogen 25.58 cubic feet, of oxygen 6.79, = 32.37; after the experiment, nitrogen 25.37 cubic feet, oxygen .67, carbonic acid 6.91, = 32.95.

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* I had previously determined, by direct experiment, that a trifling quantity of earth does not evolve any appreciable quantity of carbonic acid gas, after the lapse of twenty-four hours.

It will be observed that, whilst the mushrooms remained under the receiver, nearly the whole of the oxygen of the air, viz. 6.12, combined with the carbon of the vegetable, to form an equal volume of carbonic acid gas. The mushrooms also evolved an additional .79. Of nitrogen .21 disappeared, a very minute quantity, which may be owing partly to an absorption arising from the porosity of the mushrooms, and partly, perhaps, to an error in the experiment.

The preceding experiment was repeated by placing mushrooms, of the same kind and weight, under a receiver containing 32.37 cubic feet of air, and allowing it to remain for ten hours in complete darkness, the thermometer being at 61° Fahr. At the expiration of this time the volume of air was 32.66. The following is the result of the analysis:—The receiver contained, before the experiment, of nitrogen 25.58, of oxygen 6.79, = 32.37; after the experiment, of nitrogen 25.49, of oxygen .73, of carbonic acid 6.44, = 32.66.

From this it appears that the action of the mushrooms upon the atmosphere during the night differs little from that which goes on during the day; perhaps it appears somewhat less energetic, since it required twelve hours during the night to produce a quantity of carbonic acid somewhat smaller than that which had been produced in nine hours during the day.

The preceding experiment was repeated a third time with the species *Lycoperdon bovista*, upon three plants taken at a more advanced age, when the vegetation almost appeared to have ceased, and when this mushroom, instead of being of a fleshy consistence, appears to be only a bag full of fine powder. They, together, weighed 72 grains; the thermometer stood at 72° Fahr. Having been left for twelve hours, six during the day, and six at night, under a receiver containing 29.17 cubic feet of air, it was found, at the conclusion of this time, that the volume of air had not undergone any sensible change, and that it was much less vitiated than in the previous experiments. In fact, the receiver contained, before the experiment, of nitrogen 23.05, of oxygen 6.12, = 29.17; after the experiment, of nitrogen 23.05, of oxygen 5.25, of carbonic acid .87, = 29.17.

It is here to be remarked, that these fuzz balls, taken in the state we have mentioned, are much more liable to fermentation and spontaneous decomposition than when they are young and in a fleshy state. Does not this result serve to demonstrate that the large quantity of carbonic acid produced in the two first experiments is an effect of vegetation properly so called, and not the result of commencing decomposition and fermentation, as at the first glance might appear? In allowing that decomposition was the cause, we should have found the air more vitiated in the last experiment, when the mushrooms were nearly approximating to it, than in the former two, when they were very fleshy, and in that state in which their vitality is the greatest.

2d *Experiment.*—Three mushrooms, growing close together, of the species *Agaricus amarus*, and weighing together sixty grains, were introduced into a receiver, which contained 19.54 cubic feet of air; the thermometer standing at 68° Fahr. At the end of nine hours the volume of air in the receiver had not undergone any sensible change; its composition was as follows:—The receiver contained, before the experiment, of nitrogen 15.44, of oxygen 4.08, = 19.52; after the experiment, of nitrogen, 15.46, of oxygen .53, carbonic acid 3.55, = 19.54.

It will be observed, that in this experiment there does not appear to have been any evolution of carbonic acid already formed from any of the plants, since the quantity of carbonic acid gas which was disengaged coincides exactly with the quantity of oxygen which was absorbed.

This experiment, repeated for twelve hours during the night, afforded a result which scarcely at all differs from the above, and it is therefore unnecessary to state the details.

3d *Experiment.*—Three mushrooms, belonging to the species *Agaricus campestris*, and together weighing 190 grains, were placed under a receiver inclosing 35.58 cubic feet of air;

the thermometer being at 72°. They were then exposed to the light of day, though not in the sun's rays, for ten hours. At the expiration of this time, the volume of air had augmented to 37.33 cubic feet. The following is its analysis:—The receiver inclosed, before the experiment, of nitrogen 28.12, of oxygen 7.46, = 35.58; after the experiment, of nitrogen 28.06, of oxygen .24, carbonic acid 9.02, = 37.32.

In this instance 7.22 cubic feet of oxygen had disappeared, and gone to the formation of the same volume of carbonic acid gas. The mushrooms altogether had disengaged 2.4 inches of carbonic acid from all the plants.

The preceding experiments having been repeated during twelve hours of the night, gave a result somewhat different. The following are the details:—The receiver inclosed, before the experiment, of nitrogen 28.12, of oxygen 7.46, = 35.58; after the experiment, of nitrogen 28.12, of oxygen .73, carbonic acid 8.73, = 37.58.

It is worthy of attention, that in this instance the mushrooms appear to have commenced the evolution of the carbonic acid before having absorbed the whole, or the entire whole, of the oxygen of the air, as had occurred in the former experiments, which were conducted during the day-time.

This experiment was a third time repeated, with two plants of the species *Agaricus campestris*, together weighing 120 grains. They were first kept for twelve hours in a room, the temperature of which varied from 65° to 68° Fahr.; and were then placed under a receiver, containing 29.17 cubic feet of air, just at the time they were beginning to give out a distinct odour, the first sign of decomposition. At the end of twelve hours, the air of the receiver, the volume of which had not sensibly altered, being subjected to analysis, was found to be composed of carbonic acid 2.04, of oxygen 3·88, and of nitrogen 23.25, together 29.17. From this experiment it will be manifest that the mushrooms which had been removed for many hours from the earth, and which were on the eve of putrefaction, produced less carbonic acid than those of the same kind which were perfectly fresh; an additional reason for presuming that the considerable absorption of oxygen and disengagement of carbonic acid which we have noticed as occurring in the previous experiments, was rather the effect of the growth of the plant than the commencement of decomposition.*

4th Experiment.—A number of small mushrooms, having the shape of little blue-bells, and congregated, of the species *Agaricus digitaliformis*, weighing together 60 grains, were placed under a receiver inclosing 18.37 cubic feet of air, the thermometer standing at 72°. At the end of nine hours its volume had not sensibly changed. Analysis supplied the following results:—The receiver enclosed, before the experiment, of nitrogen 14.53, of oxygen 3.84, = 18.37; after the experiment, of nitrogen 14.43, of oxygen 1.14, of carbonic acid 2.80, = 18.37

It is here to be observed, that the mushrooms employed in this experiment are very short-lived, that they are of a very soft consistence, and begin to deliquesce in a very short time.

5th Experiment.—This experiment, which I regard as the most conclusive of the series, was made upon the *Boletus versicolor.* This species of mushroom usually grows upon decayed trunks of trees, and old stumps of wood; it is of a very tough consistence, and, even when detached from the wood to which it adheres, it exhibits no signs of decomposition for many days.

Four individuals of this variety, weighing together 140 grains, were detached from the trunk of an oak, in such a way that along with each mushroom there was removed that portion of the wood by which it was nourished. In this way these mushrooms could be subjected to experiment, without in the slightest degree altering their natural condition. They were introduced under a receiver inclosing 35.00 cubic feet of air, the thermometer being at 70° Fahr., and they were left exposed during the day-time for an interval of twelve hours. At the expiration of this time, the volume of air in the receiver had increased to 36.46 cubic feet. The result of the analysis was as follows:—The receiver enclosed, before the experiment, of nitrogen 27.66, of oxygen 7.34, = 35.00; after the experiment, of nitrogen 27.63, of oxygen .18, of carbonic acid 8.36, = 36.17.

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* In often repeating the above experiment I have remarked, that when putrefaction had decidedly commenced in the mushroom employed, there was generally a small quantity of hydrogen evolved. I have never, however, observed this result when it was fresh.

It will be perceived, that in this instance nearly the whole of the oxygen, viz. 7.16 cubic feet out of 7.34, was absorbed during the time the mushrooms were under the receiver, and had combined with the carbon of the plants, to form an equal volume of carbonic acid gas. Besides this, the mushrooms evolved about 1.02 of carbonic acid from the whole plants.

A comparative experiment, conducted in darkness for twelve hours, gave a result very nearly approximating to the above, but with this difference, that the quantity of carbonic acid formed, was somewhat more considerable during the night than during the day.

It is on purpose that, in these latter experiments, I have made the examination of the *Boletus versicolor* to succeed that of the *Agaricus digitaliformis*. The influence of these two species on the atmosphere, appears almost conclusively to demonstrate, that the absorption of oxygen and the formation of carbonic acid in the experiments which precede, did not arise, at all events in any considerable degree, from the commencement of decomposition. In fact, we find that in the case of *Agaricus digitaliformis* (see 4th Experiment), mushrooms of a soft consistence, and of such as were beginning to waste away, there was much less oxygen absorbed, and much less carbonic acid disengaged, than in the case of the *Boletus versicolor*, where the mushrooms are extremely tough, and of whose thriving vitality we can entertain no doubt, since they were removed along with the fibrous bed in which they were nourished, and so placed under the receiver.

Many other mushrooms have been successively submitted to experiment in the same manner as the preceding, but as the results did not differ from those already obtained, except as to the proportions of the oxygen absorbed, and the carbonic acid disengaged, I have thought it unnecessary to supply the details.

SECT. II. *On the Action of Mushrooms upon pure Oxygen Gas.*

1*st Experiment.*—Two mushrooms of the species *Agaricus amarus*, together weighing 90 grains, were placed, as soon as they were removed from the soil, under a receiver which contained 27.13 cubic feet of pure oxygen gas, the thermometer standing at 68° Fahr. There they remained in full day-light for twelve hours. At the expiration of this time, the volume of gas in the receiver was found to be reduced to 26.54 cubic feet. Its analysis gave the following result: The receiver contained, before the experiment, oxygen gas 27.13 cubic feet; after the experiment, oxygen gas 14.79, carbonic acid 6.27, nitrogen gas 5.48, = 26.54.

It will be seen by this result, that 12.34 cubic feet of oxygen, that is, nearly the half of what was included in the bell-glass, disappeared whilst the mushrooms remained in it. Of the 12.34 cubic feet of oxygen, 6.27 had combined with the carbonic of the plants to produce carbonic acid gas, whilst the other 6.07 appears to have incorporated itself with the mushrooms, and to have been replaced by 3.15 of nitrogen.

This arrangement, repeated during twelve hours of the night, furnished a result almost exactly the same as the preceding.

2d Experiment.—Three mushrooms of the species *Boletus versicolor*, weighing together 125 grains, were detached with care from the old trunk of an oak tree, so that a thin slice of wood remained attached to each mushroom. They were placed under a receiver containing 29.17 cubic feet of oxygen, whilst the thermometer was at 58° Fahr. There they continued during thirteen hours, being seven during the day, and six during the night. At the end of this time, the gas only occupied 26.83 cubic feet. The following is the analysis: The receiver contained, before the experiment, oxygen gas 29.17; after the experiment, oxygen gas 8.45, carbonic acid 11.96, nitrogen gas 6.42, = 26.83.

There disappeared whilst the mushrooms remained in the jar 20.72 cubic feet, that is to say, nearly three-fourths of the whole quantity. Of these 20.72 cubic feet, 11.96 combined with the carbon of the vegetables to form an equal volume of carbonic acid, which was found in the receiver; the remaining 8.76 cubic feet of oxygen appear to have remained combined with the mushrooms, and to have been replaced by 6.42 of nitrogen.

3d Experiment.—Many small clustering mushrooms of the species *Agaricus pulverulentus*, weighing 100 grains, were placed for twelve hours, nine during the day, and three during the night, under a receiver containing 24.21 cubic feet of oxygen gas, the thermometer standing at 65°. At the expiration of twelve hours, the volume of gas was found reduced to 22.75.

The analysis gave: The receiver contained, before the experiment, oxygen gas 24.21; after the experiment, oxygen gas 8.46, carbonic acid 8.17, nitrogen gas 6.12, = 22.75.

We perceive that in this experiment 15.75 cubic feet of the 24.21 disappeared. These 15.75 had been replaced by 8.17 of carbonic acid, and 6.12 of nitrogen.

4*th Experiment.*—Two specimens of the species *Lycoperdon bovista*, the fuzz-ball, weighing, the one 65 grains, and the other 35, were introduced under the bell-glass enclosing 24.50 cubic feet of oxygen. At the end of ten hours the volume of gas was reduced to 23.63 cubic feet. The receiver contained, before the experiment, oxygen gas 24.50; after the experiment, oxygen gas 8.46, carbonic acid 8.46, nitrogen gas 6.71, = 23.63.

There disappeared in this experiment 16.04 of the 24.50 cubic feet of oxygen. Of the 16.04, 8.46 reappeared under the form of carbonic acid, and the remainder appears to have entered into the mushrooms, and to have been replaced by the 6.71 of nitrogen.

Sect. III. *On the Action of Mushrooms on pure Nitrogen Gas.*

1*st Experiment.*—Four individuals of the species *Boletus versicolor*, removed from the trunk of an oak, imbedded in the wood to which they were attached, were placed under a receiver, containing 29.17 cubic feet of nitrogen, the thermometer standing at 66° Fahr. At the end of twelve hours the volume of gas amounted to 29.75. The following is the result of the analysis. The receiver contained, before the experiment, nitrogen gas 29.17; after the experiment, nitrogen gas 28.59, carbonic acid 1.16, = 29.75.

Here we find 0.58 of the nitrogen had disappeared during the time the mushrooms remained in it, and was replaced by 1.16, exactly double the quantity, of carbonic acid.

2*d Experiment.*—Two mushrooms of the species *Lycoperdon bovista*, weighing together 100 grains, were placed during twelve hours under a receiver, containing 26.25 cubic feet of nitrogen, the thermometer at 63° Fahr. The following were the results. The receiver contained, before the experiment, nitrogen gas 26.25; after the experiment, nitrogen gas 25.96, carbonic acid .58, = 26.54.

There had in this experiment, then, been the absorption of .29 of nitrogen, and the evolution of .58 of carbonic acid. This experiment, conducted for twelve hours during the night, furnished a result almost identical with the foregoing.

3*d Experiment.*—Four mushrooms of the species *Agaricus amarus* were introduced under a receiver enclosing 29.17 cubic feet of nitrogen, the thermometer standing at 66° Fahr. At the end of twelve hours the volume of gas had augmented to 30.64, and was found to consist of 29.17 of nitrogen, and .87 of carbonic acid.

In this experiment there was apparently no nitrogen absorbed, and the sole effect of the mushrooms seems confined to the evolution of .87 of carbonic acid.

The conclusions which result from this inquiry, and to which we have been conducted by the series of experiments which has been detailed, may be arranged in the following order:

1*st*, Mushrooms whilst growing in atmospherical air produce upon it changes very different from those which are effected by green plants placed under similar circumstances. Mushrooms very speedily vitiate the air, both by absorbing its oxygen, to form carbonic acid gas at the expense of the carbon of the vegetable, and also by the disengagement of carbonic acid gas, formed on all occasions when the experiment is conducted for a sufficient length of time.

2*d*, The changes which the atmospheric air undergoes from the growth of mushrooms appears to be the same both day and night.

3*d*, If fresh mushrooms are placed in pure oxygen, a great part of the gas disappears at the end of some hours. A portion of the absorbed oxygen combines with the carbon of the plant, whilst another portion appears to enter into the vegetable, and to be replaced, in part at least, by the nitrogen disengaged from the mushroom.

4*th*, Fresh mushrooms, by remaining for some hours in pure nitrogen, change very little the nature of this gas. The only effect produced is confined to the disengagement of a small quantity of carbonic acid, and in some cases to the absorption of a very trifling quantity of nitrogen.

On the Distinctive Characters of the Dog, the Wolf, and the Fox, as supplied by the Skeleton. By M. MARCEL DE SERRES

THE distinguishing of species is one of the most important points in the study of animals; and this distinction is often very difficult, especially in relation to approximating species, such as the dog, the wolf, the chacal, and the fox. In truth, the analogies which exist among these animals are so numerous and so strong, that it is only by the most accurate examination that we can succeed in distinguishing their skeletons; and the difficulty is much increased, as we are often required to determine them, at least as it regards fossil species, from the examination of mere fragments.

For the purpose of distinguishing three of these species, so closely allied, we have undertaken a minute examination, to ascertain whether the characters supplied by the form and disposition of the head, afford means of discrimination. We think we have succeeded in discovering them, and a correct judgment respecting this opinion may be formed from the following details.

The dog and the wolf form species so closely allied and connected, that many have been ready to suppose that they were identical. But this supposition will scarcely be admitted by those who believe that the species is determined by the common laws of descent. It is true that the wolf and dog may be made to propagate, but only when both are deprived of their liberty; and the offspring of this union cannot be perpetuated for an indefinite time; it cannot be prolonged beyond the third, or at most the fourth generation.

This forced and unnatural commerce, then, is never to be compared with that which uniformly takes place between individuals of the same species. We do not deny that the former sometimes takes place, but never naturally, and only under peculiar circumstances, such as the privation of liberty, and constraints of their inclinations. Such unions as these are not known to produce results similar to those which follow the play of the natural instinct, which invariably leads the members of the same species to unite among themselves for the continuance of their race, the end and condition of their existence.

The opinion has not been carried equally far in regard to the wolf and the fox. And perhaps this has been owing mainly to their native disposition presenting an insuperable obstacle to all the efforts which have been made to subdue their savage nature, and submit them, like the dog, to the dominion of man. This resistance itself proves that these species differ essentially from the dog, to which, as we have seen, one of them has been compared ; and we shall find, from the examination of the anatomical characters, that there is no real foundation for this identification with the wolf, as they do not more differ in moral qualities than in positive organization.

There is yet another species which some have wished to join with the dog, and which, like the wolf, has resisted all attempts at domestication. We refer to the chacal (*Canis aureus*, Lin.), the effect of whose capture and confinement has only been the union of individuals belonging to the two principal varieties of this species, procured from two different countries. This has been the sole result of privation of liberty on this species, which is as untameable as the wolf, with which it has been proposed to ally it, as well as with the dog. Without further proof, then, we may safely conclude, that the dog, so pliant and submissive, the inseparable companion of man, whom he respects as his master, and caresses as a friend, has nothing in common with those fierce animals with which it has been proposed to associate it.

If any doubts should still remain upon this point, they will probably be removed by the details on which we are about to enter. But, moreover, it ought not to be forgotten, that among the numerous races of dogs, none has ever been found which possessed a form and a nature at all analogous to that of the wolf and the chacal. All that is common to these species goes only to constitute them members of the same natural genus, and to exhibit those family traits which characterise the species of a single genus. We must here crave indulgence while we express regret in not having had it in our power to examine the skeleton of the chacal, which would have made the demonstration complete, and enabled us to prove, from anatomical characters, that it is with the greatest propriety that the majority of naturalists have regarded the dog, the wolf, the fox, and the chacal, as forming different and distinct species.

The observations which we now offer can scarcely be regarded as devoid of interest; they were suggested by Mr Jobert, one of the editors of the *Bulletin de Geologie*. This naturalist having honestly said, that he knew of no characters by which the skeleton of the dog could be distinguished from that of the wolf and fox, we have endeavoured to answer the appeal thus made, and have accordingly undertaken the examination and comparison of the skeletons of these animals.

Our place of residence, unfortunately, has not allowed us to give to the investigation all the perfection it might receive. At a distance from a great museum, which, from the mania for *centralization* which afflicts France, is to be found solely at Paris, we have not been able to procure those means of comparison which are indispensable. At all events, however, we have done our best to avail ourselves of the collections which have been formed by the Faculty of Sciences at Montpellier, the result of that ardent zeal which animates them in the promotion of anatomical science. When heads of different races of dogs are examined, and attentively compared with those of the wolf and fox, it may easily be observed that differences exist between them much more important than those which result from the relative size.* Thus, in comparing the heads of many kinds of the dog, and particularly those of the mastiff, the pointer, the spaniel, and the carlin, with that of the wolf, it is easily observed that the cavity of the cranium is much more developed in the former than in the latter. The distance between the two parietal protuberances is also much greater in the dog than in the wolf. But the difference in the size of the cavity of the cranium, which is so evident when we compare the dog and the wolf, is far from being equally so when we institute a comparison between the former and the fox. It is true, that in the last mentioned animal, the lateral and superior parts of the cranium formed by the parietal bones, are much more convex than the corresponding parts of the wolf; though this convexity is less, other proportions being the same, than in the higher races of dogs, especially in the most intelligent, as, for example, in the spaniel.

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* Many naturalists will hear, not perhaps without surprise, that even Linnæus, in the 12th edition of his *Species*, and Gmelin, in the 13th, distinguished the wolf, dog, and fox, by the following characters alone: tail straight or bent to the left, or downwards, of a uniform colour, or tipt with white hairs. We subjoin the very words of the great naturalist. *Canis familiaris, caudâ sinistrorsum recurvâ.—Canis—lupus, caudâ incurvatâ—Canis vulpes, caudâ rectâ, apice albo.*—Linn. Species Mamm. Ed. 12. p. 58, 59.

According to these facts, the different degrees of intelligence in carnivorous animals, as in others, would appear to depend on the larger or smaller capacity of the cranial cavity; and hence we need not be astonished to find that this capacity is smaller in the wolf, than in the dog and the fox.

The forehead is generally of a more protuberant shape in dogs than in the wolf and fox, in which indeed it is almost flat. So much is this the case, that in these latter a line drawn from the upper part of the forehead to the anterior extremity of the square bones of the nose, is nearly straight; whilst in dogs, on the contrary, the line is very decidedly curved, its greatest concavity being placed a little before the orbits.

There is also seen in the wolf a well marked depression in the middle of the frontal region. This depression is more strongly marked in some races of dogs, especially in those of inferior intelligence, as in the mastiff, which we have had such frequent occasion to mention. We have also to remark, that the lateral portions of the frontal region are distinguished in the dog by being much more prominent than in the wolf. The fox also exhibits this portion nearly quite flat. Its forehead is also proportionally flatter than that of the wolf, and it does not present that deep furrow at the junction of the crests which proceed from the posterior orbital apophysis of the frontal and forms the sagittal crest. This crest is besides proportionally less elevated and shorter, in the fox than in the wolf: and the same remark may be made in relation to the wolf and the different races of dogs.

The sagittal crest, which in the wolf is formed by the union of the two parietals and the crests proceeding from the posterior orbital apophyses of the frontal, which unite in advance of the fronto-parietal suture, is very largely developed in this animal. In those varieties of the dog, which, like the mastiff, approach near to the wolf, the sagittal crest is also developed, though not to the same extent.

Concerning the crests which arise or proceed from the posterior orbital apophyses of the frontal bone, they are not less distinctly seen in dogs, but then they are constantly rounded off. They do not unite till after they have passed the fronto-parietal suture; and it is worthy of remark, that no trace of this sagittal crest is to be found in dogs, which, like the spaniel, possess the highest degree of intelligence. It is flat, rather broad, and formed by the scarcely prominent edge of two ridges, which, proceeding from the posterior orbital apophyses of the frontal bone, run along the superior part of the parietal bones, and unite at the external occipital protuberance. In this truly intelligent race, the sagittal crest scarcely projects above the surface of the cranium, and has thus neither the same prominence nor the same development which is seen in the other varieties of the dog. It may be remarked in general, that all the crests, all the processes, and all the projecting laminæ, are much stronger in the wolf than in the dog and the fox. Hence it follows that the muscular attachments are much larger and stronger in the first than in the other two; and thus the wolf, which is much more voracious than the others, has, from this circumstance alone, better means of satisfying his appetite.

The sagittal crest projects likewise in the fox, but the crests which come from the posterior orbital apophyses of the frontal bone, constantly unite with it, at least in all the adult specimens which are under our observation, behind the fronto-parietal suture.

With regard to the occipito-parietal crest, it is usually less prominent in dogs than in the wolf, without excepting dogs of the largest size, as the mastiff. Almost no traces of it are seen in dogs which have the rounded cranium; and this difference is the more apparent when the wolf is compared with the more intelligent races of dogs; and hence it is a more distinguishing mark, as we proceed from the mastiff to the spaniel.

The line and the bony crest which run from the edge of the zygomatic arch, and pass above the external auditory foramen, are proportionally stronger and more prominent in the wolf than in the fox and the dog. As to the contraction of the cranium, which takes place behind the post-orbital apophyses of the frontal bone, it is proportionally much more distinct in the fox than in the wolf.

If we turn the crania of the dog, the wolf, and the fox upside down, and compare the transverse extent of the head, thus reversed, on a horizontal plane, this extent will be found much larger in the first of these animals than in the others. Thus we are not surprised to find the arch of the palate having a larger transverse diameter in the dog than in the wolf and fox. The basilary portion of the occipital bone, which is articulated with the posterior part of the sphenoidal bone, also exhibits a much larger quadrangular surface in the dog than in the wolf. The fox, in this particular, resembles the dog, and this gives to both these animals a larger capacity at the base of the cranium. Finally, and this character is sufficiently marked, the occipital protuberance forms a projection much more prominent, and quite otherwise developed, in the wolf than in either the dog or the fox.

The orbits of the fox are proportionably larger than those of the wolf. This is also true respecting the orbits of the dog, especially of the more intelligent races. The difference becomes more sensible as we proceed from the mastiff to the spaniel, or to any other variety which is highly domesticated. The snout of the fox is usually narrower, and more slender than that of the other animals with which we are now comparing it; so that it has almost become a proverb regarding individuals with a somewhat long, narrow, and projecting face, that they have a reynard's snout.

There are still other characters which help to distinguish the different species we are now considering; for example, the occipital foramen, which is almost circular in dogs, even in the mastiff, the variety most resembling the wolf, is, in this latter animal, of an oval form, the extremities of which answer to the inner portion of each condyle. Hence it follows that, in the wolf, the transverse breadth of the occipital foramen is greater than in the dog. In the fox also, this foramen has an oval shape, but the transverse diameter is less, other proportions being the same than in the wolf; and thus the fox, in this particular, presents a kind of mean between the other two.

The form of the maxillary bones, and the arrangement of the teeth, likewise supply very good distinctive characters. Thus the intermaxillary portion which supports the superior incisors, usually appears larger in the different races of the dog than in the wolf; and, if this distinction is not so marked in the fox, it may at least be noticed, that in it the superior incisors are more widely separated from each other than they are either in the wolf or in any variety of the dog.

The canine teeth of the dog are thicker and shorter than those of the wolf and fox. The latter of these animals is particularly distinguished by having its canine teeth very long, very acute, and probably thinner than in the dog or wolf. Their length is such, that when the incisors are in contact, the superior canines descend lower than the level of the inferior maxillary, which corresponds to them; and, in the same way, the inferior canines, in ascending, mount up considerably above the level of the floor of the nasal fossæ; appearances which are not witnessed either in the wolf, or in any of the varieties of the dog. This distinction is the more striking, as the canines in the fox have a more decided curve than the same teeth in the dog and wolf. In the young fox, the milk canine teeth are furnished, at the base of their posterior aspect, with small tubercles, which are not to be found on the permanent teeth.

The molares of the fox also present some peculiarities. The false molares so exactly cross each other, that when they are approximated, the point of the superior corresponds to the junction of the anterior and posterior edge of two inferior teeth. Thus their points nearly fill the void spaces which separate the teeth from each other. In consequence of this arrangement, these teeth are, as it were, inserted into each other, an appearance which is not presented in the dog or the wolf. In this last, in fact, the point of the superior molares projects before and beyond the point or most elevated tubercle of the inferior false molares; so that, in consequence of the oblique direction of the teeth of the upper jaw, there exists a considerable void space, lozenge-shaped, between the edges or cutting surfaces. From this arrangement it follows, that the molares of the wolf are less fitted for cutting the substances submitted to mastication than those of the fox; and, accordingly, the former of these devours more gluttonously than the latter, which is probably owing to its not having the same facilities for chewing.

The superior and inferior false grinders of the fox are, on the contrary, so arranged as to be received the one into the intervals of the other, so that scarcely any intermediate space remains when the jaws are applied close to each other. In dogs, the general arrangement of the molares is not entirely that which exists in the wolf, although they approximate much nearer to it than that which is observed in the fox. In fact, in them the interval which exists between the false molares has not the same regularity as exists in the wolf. This occurs chiefly because the false molares of dogs approach much nearer to the perpendicular than in the wolf: the grinders of this animal being somewhat oblique from before backwards, the interdental space must be smaller. The preceding characters become more conspicuous in proceeding from the mastiff to the more intelligent races of dogs; so that, from the mastiff to the spaniel, the false molares approximate more and more to the vertical position (the alveolar processes being placed horizontally), and the interdental space becomes more and more inconsiderable. The carnivorous teeth of the wolf, both superior and inferior, appear, other proportions being the same, more extended in their length from before backwards than in the dog. Thus the diameter of these teeth, in a mastiff of the largest dimensions, is from 24 to 26 millimetres, and in the wolf from 27 to 29 millimetres.* We cannot affirm that the same difference will be observable in the fox, in which the carnivorous teeth exhibit furrows and depressions more conspicuous than in the dog and the wolf, which is owing to that peculiarity which the teeth of the fox exhibit, of fitting in more deeply and accurately. Thus the triturating surface of the tuberculous teeth of the fox exhibits symmetrical excavations more deep and decided than in the dog and the wolf, so that those of the upper and lower jaw very accurately correspond in their indentations. From this arrangement it follows that the mastication of the food should be much more complete in the fox than in any of the other two animals with which we are now comparing it.

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* We have already made this remark in our "Researches upon the Fossil Bones of the Caverns of Lunel-Vieil," p. 169, in the Memoirs of the Museum d'Histoire Naturelle de Paris.

We have another observation to make relative to the tuberculous teeth of the lower jaw ;—the last, or smallest, placed behind that one which is next to the carnivorous tooth is never wanting in any jaw-bone in the wolf. On the contrary, this tooth is often wanting in the lower jaw of the dog and fox. Sometimes it exists in certain individuals, and in others there is no trace of it ; sometimes it may be seen in one side of the jaw, whilst there is no trace of it in the other. Such anomalies are never noticed in the wolf; so that the presence of all the tuberculous teeth in lower jaws, of which fragments only are obtained, is a presumption that they belong to the wolf.

Respecting the maxillary bones, whether superior or inferior, they are proportionally much shorter in the dog than in the wolf and fox. This character is evident even in those races of the dog which are largest in size, and most devoid of intelligence. The lower jaw, especially, is stronger, thicker, and fuller in the dog than in the wolf. It is also more arched towards its base, which is most strikingly witnessed when the several heads are placed upon the same horizontal plane. In this way it may easily be perceived, that, whilst the inferior edge of the jaw in the dog describes a curved line, this line is almost horizontal in the wolf, and slightly bent in the fox. Hence it follows that the elevation or the height of the sides of this jaw-bone from its base to the alveolar border is greater in the dog than in the wolf or fox.

If we place, in the position already indicated, on a horizontal plane, the head of a dog, and measure the height of the head by drawing a line from the posterior part of the backmost molares, or the tuberculous teeth, perpendicularly to the forehead, it will easily be observed that its height, other proportions being in keeping, is much more considerable in the dog than in the wolf and fox. The arched form of the jaw-bone in dogs, joined to its greater size, contribute greatly to produce this result. This character is so inherent in their organization, that it is almost as apparent in those races which are least intelligent as in those which man rears with the greatest assiduity. Nor is it useless to observe, that the distance between the two condyles of the jaw-bone is greater in the dog than in the two other animals, which is owing to the larger development of the base of its cranium.

Finally, the depression which is seen externally in the ramus of the lower jaw-bone, and which is intended to lodge the masseter muscle, is much deeper in the dog than in the wolf. Though this peculiarity may not be so striking as most of those we have already pointed out, yet it still deserves to be remembered, as it may assist in distinguishing isolated fragments, of which description specimens almost always are.

What we have now done regarding the characters supplied by the comparison of the head of the wolf and the fox with that of the dog, we might also have executed with equal advantage with the other parts of the skeleton. But we are not to forget that we meant to confine ourselves within these limits; and, besides, this subject would unquestionably be better handled by those anatomists who reside in Paris, or other capitals, where they have the advantage of great museums, and numerous objects of comparison are ever within their reach. As to what we have performed, we believe it will be found entitled to considerable confidence, inasmuch as Messrs Dubreuil, Professor of Anatomy to the Medical Faculty at Montpellier, and Dr Jeanjean, with whom we have had the pleasure of publishing various memoirs, have had the kindness, on this occasion, to verify the more important statements.—*Bibliotheque Universelle, March* 1835.

First Annual Report regarding New Inventions and Improvements in the Useful Arts throughout Europe; ordered by the Society of Arts for Scotland: Being a Report on the recent Improvements of the Carpet Manufacture. By Mr EDWARD SANG, Teacher of Mathematics, and Lecturer on Natural Philosophy, Edinburgh, M. S. A.*

THE progress of almost any of the arts may be safely taken as an index of civilization. The arts, indeed, are so intimately interwoven, that one of them can scarcely flourish without giving rise to and receiving support from others. Our farms could not be cultivated, nor their produce brought to market, unless many preparatory labours had been gone through; nor could these labours have been attempted, until the improvement of agriculture itself had procured some leisure for the human race. In like manner all the arts mutually receive support from and assist each other; so that, in attempting to view the present state of society, we would need to consider the arts, not in detached pieces, but as one vast and compact whole. But if our object be to compare one age with another, and to ascertain the rate of civilization, as it were, we may easily select departments which, receiving active assistance from many others, may indicate with sufficient nearness the general state of the whole. There are arts, such as the manufacture of glass, which have, as yet, depended so slightly upon others, as to afford no criterion of the general progress. Our ignorance of high temperatures, and the incorrodible nature of glass itself, have prevented this important manufacture from receiving that powerful assistance which chemistry might have been expected to have yielded to it: the method of forming glass vessels has thus remained almost unchanged, and still entirely depends on the strength and dexterity of the workman.

Not so with the manufacture of carpets: this, like the other branches of weaving, has received improvements at every hand, and has lately made important advances. The very fact of the existence of such a manufacture speaks volumes as to the increase of our domestic comforts. But a few generations back, the royalty of England was contented with that fare which we now confine to our stables, and lands were granted for the servitude of providing *clean straw* for the royal chambers. Well scoured deals and good Kidderminsters have now displaced that vehicle of infection, and that not merely in the houses of the wealthy, but even in the dwellings of the industrious artizan. To some, the introduction of such luxuries may appear to savour of effeminacy; those who admire nations only when formidable in war, who glory in bloodshed and devastation, may fear that, when each peasant treads upon a well-made carpet, when the cot boasts of luxuries unknown two centuries ago, even in the palace, nations will lift but feeble hands in encroachments upon each other. But he is much mistaken who regards such comforts as marks of effeminacy. Were they the results of Roman spoliation,—were our furniture, our china, and our carpets, torn from the industrious of conquered provinces,—had we our silks for tribute and our cottons for booty, then indeed would we be effeminate and vicious. But those comforts which we so richly enjoy are the results of industrious and upright toil. Every luxury that surrounds us is the fruit of skill and energy: the procuring of it has exercised our patience and improved our strength, while the enjoyment of it restrains us from wanton aggressions on our neighbours, and nerves us in our own defence. The spread of comforts among every rank, guards us against tumult on the one hand and oppression on the other; and every improvement on our manufactures, every cheapening of our commodities, is a step forward in the path to happiness and security.

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* Read 12th August 1835.

It is not merely by its existence that the carpet-manufacture bears testimony to the advanced state of society; the elegance of its processes, and the beauty of its products, speak a language no less distinct.

In the superficial texture of the common carpet, nothing appears to distinguish it from an ordinary web; and a first observer is at a loss to imagine by what means its variety of colours can be produced. On examining the figure more narrowly, it appears that the designer has laboured under considerable difficulties; for in many places where purity of colour would have been advantageous, a mixed colour only is to be found, while scarcely any gradual shading of the tints depending on the nature of the figure is to be seen. A still closer examination explains at once the source of these imperfections. The carpet is found to consist of two contiguous webs, intermingled with each other in such a manner as to produce the pattern: each of these webs, if woven singly, would have a striped appearance, being parti-coloured in the woof. One set of coloured stripes is thus imposed upon another; and in designing the colours of the pattern, no selection beyond what is afforded by the judicious arrangement of these stripes can be made. The number of full colours is thus very limited: these can only be obtained where woof traverses warp of the same colour. To bring up then a part of the figure full red, red warp of the whole breadth must be traversed by red woof of the whole length of the spot; these colours can be immediately concealed by sending the threads to the other web, but were they to remain long there, both webs would become monotonous. It is therefore extremely difficult to avoid a strong tendency to striping in the colours, and, except on the principal part of the figure, the colours can hardly be well managed, the secondary embellishments being almost matter of chance. Yet in the face of all these difficulties, patterns of great beauty are daily formed on the carpet loom.

The invention of the *triple carpet* by Mr Morton of Kilmarnock, has almost removed these difficulties. This carpet is composed of three webs, which interchange their threads in order to produce the pattern. The primary object in the introduction of the third web, appears to have been the obtaining of greater variety and brilliancy of colouring; but another curious effect has followed, that the two sides of the carpet are not necessarily counterparts to each other. To a certain extent the figure of the under must depend on that of the upper side, since threads may be needed from the under web to produce what is wanted in the chief pattern, but there still remains the choice of an interchange of threads between the two inferior webs. It is obvious that the tendency to striping must be much less on this than on the common carpet, and that the designer having a far greater choice of colours, may produce effects that could not before have been attempted. It appears to me that, after the principal figure has been determined on, the skill of the designer is most severely exercised on the wrong side of the carpet. His choice of materials is indeed as great as with the common carpet, but then he is hampered by the restriction in figure, and can only be entirely at ease opposite a flat piece on the right side. The beauty of the triple carpet is at once acknowledged: it possesses almost all the freedom in colouring of the floor-cloth or paper-hanging, while its greater thickness and comparative cheapness bring it into competition with the more expensive kinds of carpeting.

The inventor of this texture has conferred on us a very great benefit: he has furnished us with a higher embellishment for the interior of our dwellings, and presented to us another evidence of the active benevolence and social disposition of man. And it is agreeable to reflect that, in the nursing of the idea, and the carrying of it into effect, he must have felt a pleasure much more intense than is likely to be experienced by any of the multitudes who will enjoy the fruits of his abilities.

A desire for something in the interior of a dwelling analogous to the soft clothing of the external world, seems to be generally felt; for in all states of society attempts are made to remove the hardness and unseemliness of the floor. Among the poorer nations, these attempts are confined to the mere dormitories; but, as advances are made in wealth, the mat and the carpet begin to appear. The softness of the turf and more than its smoothness having been attained, it was natural to imitate also its embellishments: for this purpose several distinct kinds of carpet texture have been contrived. On one of these I have already reported an immense improvement, and proceed to describe a no less striking improvement on another.

The Brussels carpet is distinguished from the common one by having a raised pile, and by the circumstance that the figures and colours are entirely produced from the warp. The pile is raised by inserting a wire between the body of the warp and the previously raised colouring threads. These threads descend and are fixed by the woof, and after a few repetitions of the process the wires are withdrawn. The Wilton carpet differs only in this, that the pile is made somewhat longer, and cut in the manner of velvet. Were the coloured warp, however, raised into pile at each stroke, the web would have simply a striped appearance; and if it were raised only at intervals, the figure would be given in relief, but would still be merely striped. In order to produce a properly coloured pattern, several coloured yarns are arranged, so that any one of them may be raised into pile between the same two permanent warp threads. Their number is generally five, so that, by their irregular ascent to the surface, the striped appearance is almost broken up. Still, however, the web is essentially striped, and though the designer be not nearly so hampered as in the Kidderminster texture, he is still seriously incommoded in his choice. Let us suppose a board painted in minute coloured stripes. After these have dried, let another coating of coloured stripes be laid on, and so for five coats, each differing from the preceding: the painter may now form an idea of the difficulties encountered by the carpet designer,—let him set to work, by scraping away the different coats, to produce a pattern. But there is another annoyance; in order to produce the smallest speck of any particular colour, a thread of that colour must traverse the whole pattern; and that thread may displace some other which would have been advantageously brought in elsewhere. On account of the very different rates at which the coloured threads are taken up, these cannot be wound on one beam, but have to be placed each on a bobbin by itself.

To remedy the inconvenience of this texture, Mr Whytock contrived his method of partially dyeing the yarns; but we cannot fully understand the value of the contrivance till we have glanced at another kind of carpet texture.

The Turkey carpet is the simplest in its texture of all the carpets, and at the same time is almost unlimited in the choice of colours. Let us suppose ourselves seated at a common loom, and that immediately after having thrown a shot we commence to tie on every thread of the warp a small bunch of coloured worsted, varying the colour according to our fancy. This completed, let two or three shots be thrown, and well driven up; and then another row of coloured worsteds tied on. It is clear that in this way we could produce any pattern, and that no more of any particular colour is wanted than is sufficient to produce the required effect: nay more, the colours being put on by hand, we would not be compelled to reiterate the pattern at each stated distance. Here we have every advantage that we can wish for, excepting this important one, rapidity of formation.

Mr Whytock's method supplies to all the advantages of the Turkey carpet, a rapidity of weaving greater than that of the Brussels fabric. His method may be described thus: If for the five coloured yarns of the Brussels carpet we could substitute one yarn dyed of the requisite colour at different places, we would be able to dispense with all the apparatus for producing the pattern, could make the web with only one body, and work it as a simple velvet. The only difficulty would then be in the dyeing of the thread.

In order to dye the thread, one yarn is wound on the surface of a large drum, of which the circumference is equal to the length required for one copy of the pattern. This drum is graduated so that the dyeing roller can be passed across the yarn at any required place. The design, extended on the ordinary ruled paper, enables the workman to discover all the places at which a particular colour is to be applied: that done, he changes the colour box, and so proceeds till the whole colouring is completed. The thread is then taken off the drum, and submitted to the processes for fixing and bringing up the colours. The next thread is then dyed, and so on till the whole warp is finished. The next and the most difficult process is, to place all these yarns side by side upon the beam. For this purpose they are wound upon separate bobbins, and small white spots, purposely left in the dyeing, enable the workmen to arrange the coloured parts properly opposite each other. They are then carefully rolled upon the beam, and the weaving proceeds rapidly, each thread being brought into the pile upon every successive wire. Mr Whytock uses the grooved wires, and cuts the pile in the manner of the Wilton carpet.

Excepting in the necessity for the recurrence of the pattern, this has all the advantages of the Turkey carpet. The coloured spots can be produced at any point, and need not run in rows as in all the others. It need hardly be added, that greatly admired patterns have already been produced by this method; and that the manufacture meets with great and deserved encouragement.

Before concluding this imperfect notice of these two improvements, I would draw attention to a subject of great importance to society in general. A strong prejudice, sanctioned by an old proverb, exists against those who turn their attention to several branches of the arts. Yet it is a fact, that almost every improver has been jack of a good many trades,—nay, an acquaintance with a variety of operations is essential to the invention of new ones; and very often prodigious improvements are effected by the simple transference of a process from one art to another. May I be allowed to hint, that the triple carpet is one of those generalizations so often found in scientific researches, and that its inventor appears to have extended his studies far beyond the subject of carpet weaving. The patent carpet, again, bears on the face of it the necessity for a knowledge of the arts both of dyeing and of weaving; for no one not intimately versed in both arts could have conceived, or, having conceived, could have carried the idea into effect. The difficulties to be encountered were by no means few, and the overcoming of each was itself an invention.

Another idea exists, that the happening upon new discoveries is a matter of chance; and some appear (I judge from their conduct) to imagine that the less they know of a subject the more likely they are to alight on something new—as a bad swordsman trusts to that very circumstance for outwitting his antagonist. Once in a century indeed one man among ten millions may find, by chance, some valuable process; but the great mass of our current inventions are the fruits of assiduous and well directed exertion; and the mind, even more truly than the body, must earn its food in the sweat of its brow.

EDINBURGH, 12*th Aug.* 1835.

On the Vegetables which are found in the Thermal Mineral Waters of Abano, and in their immediate vicinity. By Dr ANDREJEWSKYI.*

THESE waters are situated in the valley of the Po, not far from Padua, and on the western slope of a small group of volcanic cones, denominated the *Monti isolati* by the inhabitants. They rise very abruptly from the lowest part of the valley. The spring we are now about to introduce to the notice of our readers, has its origin at the very summit of one of those little cones, known under the name of Montiron. The waters vary in temperature from 122° to 145° Fahr., and contain soda, lime, magnesia, alum, and the oxide of iron, also sulphuric acid, silicic acid, chlorine, iodine, and bromine, differently combined.

The phanerogamous plants which grow nourished by these waters, rarely attain their usual height, and, in truth, are always more or less stunted. Thus the *Althea rosea* in flower is not above a foot high; the *Hypericum perforatum* is 2 inches and 8 lines; the *Ajuga chamœpytis*, 1 inch and 5 lines; the *Sedum acre*, 1 inch 3 lines; the *Spergula arvensis*, 2 inches; the *Arenaria rubra*, 3 inches 3 lines; the *Mentha arvensis*, 5 inches 6 lines; and the *Adianthum Capillus Veneris*, and the *Mnium* and *Bryum*, grow most miserably. The plants which really flourish in these localities, are those which thrive in the neighbourhood of water; and the four first which we enumerate are found in the marshes about Venice: these are the *Salicornia herbacea*, *Samolus Valerandi*, *Juncus acutus*, *J. maritimus*, *Atriplex veneta*, *Sonchus maritimus*, *Aster Tripolium*, &c. The soil in which these plants are produced is a tuffa, evidently of volcanic origin.

It is not to be supposed that all these plants are watered by the spring at 145° Fahr.; for it will be at once apparent, that the temperature will be diminished in proportion as the vegetables are situated at a distance from the fountain-head. Nevertheless the *Samolus Valerandi* grows vigorously in water whose

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[*] Extracted from the Journal der Chirurgie und Augenheilkunde von Graefe et Walther. 4to. Cah. 1831.

temperature is at 152°, and the *Zanichellia palustris* in water at 95°.

Besides these phanerogamous plants which flourish around the spring, there are found in the mineral waters themselves a vast number of plants belonging to the great family of the Algæ. Even Pliny has taken notice of this, and many other authors have mentioned it since his day. Thus Claudien says, in speaking of these springs, "*Fumantia vernare pascua luxuriare coctam silicem;* and Cassiodorus, in the letter of Theodoric to Aloys the architect, thus expresses himself: "*Rideat florenti gramine facies decora campestris quæ etiam ardentis aquæ fertilitate lætatur miroque modo dum proxime salem generat sterilem nutriat pariter et virores.* Eight centuries after Cassiodorus, the Marquis Jean Dondi found the stones covered over with vegetation, and Fallopius witnessed these plants loaded with flowers and fruit. Baccius and Vallisneri have alleged that the Confervæ were only found at the junction of the cold water with the hot, along the margin of the stream; but this statement has been refuted by Vandelli and Mandruzatto.

Vandelli, in his *Tractatus de Thermis agri Patavini*, 1761, describes the following Confervæ:

Conferva alba.—In the mineral waters of Saint-Pierre, where the temperature is at 145° of Fahr., the white colour is owing to the high temperature; for, when found in water at 122°, the plant has a greyish colour.

Conferva anonyma.—These appear as white filaments, which gradually form a crust on tuffaceous matter. They occur in water at 122°.

C. Aponitana.—These are triquetrous leaves, furnished with vesicles, like those of the marine algæ. They occur in water at 118°.

C. capillacea.—In water at 112°.

M. Andrejewskyi has carefully observed the *Ulva labyrinthiformis*, Lin., which grows abundantly in these waters. Linnæus arranged this plant among the Algæ; Agardh, among the Oscillares; Bory de Saint Vincent, also among the Oscillares, and in the genus Anabaina, which forms a part of the scale of living beings, intermediate between the vegetable and animal kingdom.

This plant is found among the springs of Abano, in places where the temperature varies from 133° to 145° Fahr. In spring and the beginning of summer, it forms a floating membrane, of a beautiful green colour. It has somewhat the appearance of jelly, slips along the fingers, and is covered with a great number of very small vesicles, filled with some gas or air. At a later period, it is covered with a gritty powder, and sinks into the water; there then rises from these vesicles projections in the form of hollow quills, which rise as high as from two to four inches. Their heads are roundish, and surrounded with a crown of small appendices. In autumn, or at the commencement of winter, these small heads break open, the gas which filled them escapes, and the whole mass sinks down. These projections finally are crusted over with a gritty powder, assume a reddish colour, and it is only in some places that the green colour remaining indicates their vegetable origin.

The author has not been able to discover the organs of reproduction. After macerating the plant for a long time in water with muriatic acid, he got rid of the stony deposit; and M. Link could distinguish, with a magnifier of four hundred powers, very delicate and confused filaments, at the edges of which were small canals, containing a red juice. Upon analysis, this plant afforded sulphuric, silicic, and carbonic acids, chlorine, iodine, sulphur, ammonia, soda, lime, magnesia, alumina, and iron.

On the Animals designated in the Scriptures by the Names of Leviathan *and* Behemoth. (Communicated by the Author.)

While every sentence in the writings of the Greeks and Romans, having any reference to any branch of science, has been illustrated with great learning and profound research, men of science do not appear to have turned their attention, with that interest which the subject might well inspire, to the illustration of similar passages in the original and authentic archives of the human race, contained in the writings of the Hebrews. Several causes may be assigned for this, among which, no doubt, the circumstance is to be accounted the chief, that the language of the original does not form a common subject of study at our schools and colleges, like the Greek and Latin, although the richness of the treasures contained in it,—the faithful history and painting there of ancient manners,—the noble tone of a steady and just morality, which pervades the whole series of a literature originating with the first of men, and enlarging through many succeeding generations,—the sublime and rational theology, forming a sanction to that morality,—and the deep influence which all these have exercised upon our own English literature, through the instrumentality of the popular diffusion among us of the common translation of the Bible,—might well have enticed all who possess the necessary attainments, to the illustration of every thing contained in writings so remarkable, and which, the more they are studied and compared together in their various branches, the more they exhibit the infallible impress of consistency and truth.

A better spirit with regard to this matter is now displaying itself among the cultivators of science; and I perused lately, with interest, "An Attempt to ascertain the animals designated in the Scriptures by the names *Leviathan* and *Behemoth*, by Thomas Thompson, Esq. one of the vice-presidents of the Hull Literary and Philosophical Society."

Mr Thompson arrives at the result, that by *Leviathan* is meant the Megalosaurus, a predatory lizard or saurian, 60 or 70 feet long, the remains of which are found chiefly in strata which lie below the chalk; and that by *Behemoth* is meant the Iguanodon, an herbivorous saurian, upwards of 60 feet long, whose remains were found by Mr Mantel below the chalk, but very close to it. I conceive Mr Thompson's conclusions on the two questions to be erroneous, and shall set down some of what appear to me the most decisive proofs that they are so.

In order that Mr Thompson may be free to apply the term *Leviathan* to the Megalosaurus, it becomes necessary for him to adopt the Reverend James Hurdis' conclusion, that the Hebrew word *Than* or *Thanin*, means the Crocodile which still inhabits the Nile. It is even indispensable to his reasoning, that we understand that Hebrew word as the *specific* name of the Crocodile; for, in page 307, he says, "Already we have seen reason to assert that the *Than* of the Scriptures is the Crocodile. Then the Leviathan could not also be the Crocodile; for both animals are distinctly mentioned in the same portions of Scripture."

Now, it can admit of no doubt that the name *Than* does sometimes in Scripture mean the *Crocodile of the Nile;* as in Ezek. xxix. 3, quoted by Mr Thompson,* and in some other passages. But we should take a very erroneous view of the use of the term, if we supposed that the Hebrews applied it to that reptile alone. We find it applied to poisonous serpents, as well as to saurians or ophidians of regions so dry, that the Crocodile, from its habits, could not be an inhabitant of them. Of this some instances shall be noticed. Deut. xxxii. 33, "Their wine is the poison of *Thaninim*, and the cruel venom of *Pethanim*." Mr Thompson has himself quoted one passage, from which we gather, that some of the creatures named *Thanim* were inhabitants of a dry country. It is, Jer. ix. 11, "I will make Jerusalem a heap, a den of *Thanim*." A passage of like effect is Jer. x. 22, "To make the cities of Judah desolate, a den of *Thanim*." We find a well at Jerusalem, which has got its name from *Thanim*—a circumstance which could not have probably occurred, unless the creatures so named had frequented the neighbourhood. Neh. ii. 13, "I went out by night by the gate of the valley, even before the *dragon-well* (oin hathanim), and viewed the walls of Jerusalem." And in Mal. i. 3, we have the *Than* described as existing in one of the driest regions of the earth, and the most destitute of rivers, but which we know is infested with small lizard and serpent tribes; "and I hated Esau, and laid his mountains and his heritage waste, for the *Thanoth* of the wilderness."

It is apparent from these examples, that the term *Than* or *Thanin* was used by the Hebrews as a sort of what we would now call an ordinal or classical name, to designate both ophidian and saurian tribes; and thus although it was sometimes used to designate the Crocodile of the Nile, among other species, it was by no means applied to it exclusively.

The only exception to this application of the term to the ophidians or saurians, is that referred to in an editorial note, appended to Mr Thompson's paper. It is in Lam. iv. 3, where the

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* Ezek. xxix. 3, "Behold I am against thee, Pharaoh, king of Egypt, the great dragon (hathanim hagadol) that lieth in the midst of his rivers," &c.

Than is described as giving suck to its young ones. But the whole book is highly poetical; and we might be at liberty to imagine a poetical licence. Without that we must reflect that it would have been only in consistency with what takes place in all countries, had the later Hebrews designated an animal, somewhat like a reptile, for which their ancestors had no name, as a seal, or one of the whales, by a name for a reptile long established in their nation; and such was the term *Than*, for it occurs so early as in the first chapter of Genesis.

The term *Than*, then, was not a name confined to the Crocodile of the Nile alone. But that saurian so much surpassed in size all others known to the Hebrews, and was so distinguished from them by a strength and ferocity dangerous to man, that we might naturally expect they would have a specific name for it; and such a name, I will now briefly shew, they had in *Leviathan*. I would refer in proof of this to Isa. xxvii. 1, which Mr Thompson has quoted with a contrary view. It is quite obvious from the concluding verses of the chapter, where Egypt is expressly named, that, in the opening of his subject, in the beginning of the chapter, announcing the punishment of Leviathan and Thanin, Isaiah refers to an inhabitant or inhabitants of the Egyptian river then existing. Will it be said that the Megalosaurus existed in the Nile, so lately as the time of Isaiah,—about 300 years only before the age of Herodotus? Mr Thompson will see the force of this objection to *Leviathan* being the Megalosaurus; for to obviate a similar objection in reference to the Leviathan in Job, he observes, " that the period of Job's existence is uncertain, but most critics think he lived long prior to the time of Moses: possibly even before the flood."—(P. 319).

But it is quite in accordance with a phraseology very common in Hebrew poetical writings, such as those of Isaiah, that we should understand both *Leviathan* and *Thanin*, in this verse, to mean the same animal. It is common in these writings, when any thing is predicated in them of any subject whatever, for which the writers had more than one name, that two or more names are introduced in the course of the predication, and names quite equivalent to each other, and definitions of subjects are multiplied in them in the same manner as names.

Here, for instance, in this verse itself, putting *Thanin* quite aside, *Leviathan* is named both that *piercing serpent* and that *crooked serpent.* Isa. xxvii. 1, "In that day the Lord, with his sore, and great, and strong sword, shall punish Leviathan that piercing serpent, even Leviathan that crooked serpent, and he shall slay the Thanin that is in the sea." Many examples of a similar phraseology might be quoted; but it is most satisfactory to take some from this prophet himself. We have several even in this twenty-seventh chapter, of which the most in point is in the 11th verse, "He that made them will not have mercy on them, and he that formed them will shew them no favour." The Hebrews have several names for the lion, and this prophet introduces two of them together in chapter xxx. 6, "The burden of the beasts of the south: Into a land of trouble and anguish, from whence come the young lion" (labia), "and the old lion" (laish); where there is no authority for the *young* and *old* of our translators. The examples of this manner of speech in Isaiah are endless; and I shall set down only two more that open up in the same pages with the verse under discussion. Chapter xxv. 2, "For thou hast made of a city an heap, and of a defenced city a ruin;" and verse 7th, "And he will destroy in this mountain the face of the covering cast over all people, and the vail that is spread over all nations."

It can be still more readily demonstrated, that in Psalm LXXIV. 13, 14, quoted also by Mr Thompson, both *Thanin* and *Leviathan* are intended to express the same animal. The poet here obviously refers to the passage of the Red Sea, and the destruction of the Egyptians there; so that Thanin and Leviathan are both types of Egypt. That both terms express the same animal becomes obvious from attending to the structure of the different clauses of the poem, in which, from the beginning, there is frequently a repetition of a thought expressed in varied terms. Thus, in verse 2d, "Remember thy congregation which thou hast purchased of old," is repeated, "the tribe of thine inheritance which thou hast redeemed." Verse 9th, "We see not our signs, there is no more any prophet," is followed by the equivalent, "neither is there among us that knoweth how long." Verse 10th, "How long shall the adversary reproach," is repeated thus, "shall the enemy blaspheme thy name for ever?" How obvious does it thus become, that " Thou brakest the heads of the *Thaninim* in the waters," is a thought repeated in the terms which follow, " Thou brakest the heads of *Leviathan* in pieces."

In the progress of our investigation, then, we have found *Than* unquestionably employed by Ezekiel to express the Crocodile of the Nile, and *Leviathan* employed both by Isaiah and the author of the seventy-fourth Psalm, synonymously with *Than*, in passages where both these last authors professedly refer to Egypt or the Egyptians. We have also seen that *Than* is frequently used to express other saurian or ophidian races. But we would now observe, with respect to *Leviathan*, that there is no passage in which we can gather, from the context, that it means any other animal than the crocodile of the Nile.

The only other passages in which the term occurs are Psalm civ. 26, and Job, chapter xli. In the former, *Leviathan* is an inhabitant of the waters. In Job he is an inhabitant of the waters, and sometimes of the pebbly beach; and we shall now proceed to shew, that, while the description in Job admirably agrees with the Crocodile of the Nile, the objection urged by Mr Thompson, against allowing that saurian to be signified there, falls to the ground, when we take in all the conditions of the question.

But indeed, in shewing that the characters of Leviathan, in Job, are those of the Crocodile of the Nile, Mr Thompson has already exhausted the subject; and we need only to give an outline of his detailed comparison. The scales of the Leviathan, forming a close and impenetrable barrier, are those of the Crocodile, which, according to Griffith's Cuvier, " is covered with small bucklers, which are proof against the sword and musket ball." The terrible teeth, the glancing of the eyes, the fume from the nostrils resembling smoke, the might of the neck, the movements occasioning great agitation of the waters, the great strength, and frightful presence of the Leviathan, Mr Thompson acknowledges to belong to the Crocodile, and only claims that they may have belonged to the Megalosaurus in a more eminent degree.

His objection to admitting the *Leviathan* to be the Crocodile is derived from the admission, in Job, of the impracticability of capturing or destroying him; while we gather from Ezekiel, that the *Thanin* might be caught with a hook, and learn from Herodotus and Pliny, that the Crocodile of the Nile was in fact so caught, and by other means, by the Egyptians of their time; and while we know also, that at the present day it is speared and slain by the natives of the countries it inhabits. But the objection loses all force when we reflect, that the book of Job was written many ages before the time of Ezekiel and Herodotus. He must have been a fearless champion who first adventured to assail or capture one of the most formidable of animals, and the best protected, that we know, against piercing or cutting weapons; and the arts of subduing that animal, afterwards known, may not have been known in the time of Job. Mr Thompson reasons, that the impracticability of capturing the Leviathan by a hook, or securing it by a bridle, declared in Job, refers to the means described by Herodotus and Pliny of capturing the crocodile by a hook, or a stick thrust crosswise into its mouth, like the bit of a bridle; and that, therefore, as Job was aware of these means, and asserts the Leviathan could not be captured by them, the Leviathan must have been some more formidable animal than the Crocodile, such as the Megalosaurus. But we are not entitled to adopt this reasoning; for the mentioning of the *hook* in Job obviously refers to the peaceful and little hazardous arts of the fisherman, fish-spears being expressly mentioned in Job, chap. xli. 7, along with the hook. The reference to the bridle is no doubt to the implement which secures the horse, an animal, the training of which, even to war, was well known to the author, chap. xxxix. 19–25; and the bridle itself is mentioned chap. xxx. 2.

As to the time when the book of Job was written, a point now of great importance in our inquiry, there is no external historical evidence which enables us to fix it with certainty. There is, however, even a less suspicious form of evidence, that which is internal of the book itself, and of the other Hebrew books which have been transmitted to us along with it, which will enable us to determine the time within certain limits. That it was written posterior to the flood of Noah is evidenced by the clear reference to that sudden and unexpected catastrophe, and to the impiety of the antediluvians, in chap. xxii. 15, *et seq.*

There is also a reference to the fate of the cities of the plain of Jordan, in the 20th verse of the same chapter. The date is, then, later than the time of Abraham. On the other hand, we have clear evidence that it was earlier than the time of Moses. The book of Job is so consistent in design, and so complete in execution, in its beginning opening up, and in its conclusion finishing one great argument, as to carry on its face the impress of an original work, the production of one great and highly poetical mind. The phraseology and structure of all the sentences are unique, and there is a very slight but uniform dialectic difference between it and the other Hebrew writings of the Old Testament. When we find remarkable coincidences, in images, forms of expression, and sentiments, brought out in the same order, between passages in this book and others in the briefer hymns and poems of other Hebrews, we can have no reason to doubt, from the above characters in Job, that it is the original and the others imitations. When we come down to the great Hebrew lyric poets, David and Solomon, we find in their poems such innumerable coincidences with Job, as leave no doubt they had that book in their hands as a familiar manual, filling their minds with poetical images, and forming a model for their *phraseology*, just as the Greeks had Homer, and the English now have Shakspeare and Milton. When we trace this matter up to an earlier date, we discover also, in the few poems of Moses which have been transmitted to us, coincidences with passages and images in Job, which can be accounted for in no other way but by allowing that Moses also had the book in his hands. We cannot here enter into this matter in all the detail of which the subject is worthy; but let any one compare the following passages of Job and Moses, and he will see the conclusion is inevitable, that the book of Job was familiar to Moses; Job iv. 9, " By the blast of God they perish, and by the breath of thy nostrils are they consumed;" with Exodus xv. 7, 8, " Thou sentest forth thy wrath, which consumed them as stubble, and with the blast of thy nostrils the waters were gathered." Job viii. 8–10, " For inquire, I pray thee, of the former age, and prepare thyself to search of their fathers: shall not they teach thee and tell thee?" with Deut. xxxii. 7, " Remember the days of old, consider the years of generation and generation: ask thy father, and he will shew thee, the elders, and they will tell thee." Job xxix. 6, "When I washed my steps with butter, and the rock poured me out rivers of oil;" with Deuteronomy xxxii. 13, 14, "He made him to suck honey out of the rock, and oil out of the flinty rock; butter of kine." Job xiv. 17, "My transgression is sealed up in a bag, and thou sewest up mine iniquity," with Deuteronomy xxxii. 34, "Is not this laid up in store with me, and sealed up among my treasures?" in which last it appears from the context, that it is the iniquity of the heathen that is sealed up in the treasures of God, waiting vengeance and recompence. The image is the same in Job in reference to his own sins. We have satisfactory evidence, then, that Job was written prior to the time of Moses, independently of the great argument of all the commentators in behalf of that position, "that there is in Job nowhere any allusion to the Exod of the Israelites, or the peculiarities of the Mosaic institutions."

Thus, then, we learn that the book of Job was written at least nearly a thousand years before the time of Ezekiel, and twelve hundred before that of Herodotus. Would it, then, be more safe for us to hazard the conjecture, that the author of Job was acquainted with the arts of destroying the Crocodile, known to these later writers, than it would be consistent with the fact to assert, for instance, that Pliny was aware of the art of capturing the northern whales, because, a thousand years after his time, we find the Basques practising that art?

The only objection thus failing, we arrive at the conclusion, that the *Leviathan* of the Scriptures is the Crocodile of the Nile. We would add a concluding remark in reference to that term, which is, that it is not the only Hebrew one into which the more extensively applicable term *than* enters as a part. We have found another in Deut. xxxii. 33, *Pethan* or *Phethan*, which is not of unfrequent occurrence in other passages. A poisonous power is often ascribed to *Pethan;* and as it may be interpreted *than of the mouth*, it may have got its name from the circumstance of its inflicting its venom by its bite. As to *Leviathan*, that is strictly *joined* or *coupled than;* and the name may have a reference to the manner in which the scales, or mail-plates, of the Crocodile are joined together, as so admirably described in

Job xli. 15, 16, 17. I am not aware that there is any evidence of the Megalosaurus having possessed similar armour.

With respect to *Behemoth*, I will not venture to assert that we shall arrive at the same assurance what species of animal is meant by the term in Job; but, upon the plainest and fairest principles of critical interpretation, we can demonstrate, that no saurian of any kind is meant, but one of the larger herbivorous mammalia. *Behemah*, plur. *Behemoth*, is the very term which the Hebrews use to express these, as can be proved by satisfactory references; as, for example, Levit. xxvii. 9, " If it be a *beast* (behemah) of which men bring an offering unto the Lord." The Hebrews offered only beasts that divide the hoof and chew the cud. Gen. xxxiv. 23, " Shall not their cattle, and their substance, and all *beasts of theirs* (behemetham), be ours?"

The author of Job was conversant with the records of the creation in Genesis, and with the histories of Adam and of Cain and Abel, as is demonstrated by obvious allusions which he makes to them. For example, among many others that might be referred to, the following passages clearly establish that fact. Gen. i. 26, " And God said, Let us make man in our image, after our likeness," &c.; verse 27th, " So God created man in his own image, in the image of God created he him." Gen. ii. 7, " And the Lord God formed man of the dust of the ground, and breathed into his nostrils the breath of life; and man became a living soul." These passages are clearly alluded to in Job x. 8, " Thine hands have made me, and fashioned me together round about;" verse 9th, " Remember, I beseech thee, that thou hast made me as the clay; and wilt thou bring me into dust again?" Job xxvii. 3, " All the while my breath is in me, and the breath of God is in my nostrils." Job xxxii. 8, " But there is a spirit in man; and the inspiration of the Almighty giveth them understanding." Job xxxiii. 4, " The Spirit of God hath made me, and the breath of the Almighty hath given me life." Gen. i. 6, 7, " And God said, Let there be a firmament in the midst of the waters, and let it divide the waters from the waters. And God made the firmament, and divided the waters which were under the firmament from the waters which were above the firmament." Alluded to in Job xxvi. 8, " He bindeth up the waters in his thick cloud, and the cloud is not rent under them." Gen. i. 9, "And God said, Let the waters under heaven be gathered together into one place, and let the dry land appear." Alluded to in Job xxvi. 10, "He compasseth the waters with bounds;" and in Job xxxviii. 10, 11, "And brake up for it (the sea) my decreed place, and set bars and doors; and said, Hitherto shalt thou come, but no further, and here shall thy proud waves be stayed." This last allusion in Job to the work of the third day, or period of creation in the first chapter of Genesis, is followed, in the 12th verse, by an allusion to the work of the fourth day, or period of creation in which the sun and moon were appointed for days and for years. Verse 12th, "Hast thou commanded the morning since thy days, or caused the day-spring to know his place?" Gen. iii. 8, 10, "And Adam and his wife hid themselves from the presence of the Lord God." "And he (Adam) said, I heard thy voice in the garden, and I was afraid, because I was naked; and I hid myself." Alluded to in Job xxxi. 33, "If I covered my transgressions as Adam, by hiding mine iniquity in my bosom." Gen. iv. 10, 11, "And he said, What hast thou done? The voice of thy brother's blood crieth unto me from the ground. And now art thou cursed from the earth, which hath opened her mouth to receive thy brother's blood from thy hand." Alluded to in Job xvi. 18, "O earth, cover not thou my blood, and let my cry have no place."

The detail of these allusions may appear tedious; but the establishment of their existence and frequency in Job brings a clear light upon our present inquiry. The *Behemoth* in Job is introduced by an allusion of the same kind to the same early records. Job xl. 15, "Behold now Behemoth, which I made with thee." This is an obvious allusion to the work of the sixth day, or period of creation, in the first chapter of Genesis, in which man was created, and in which also *Behemah* is expressly named, along with other mammalia, as being created. It is, indeed, mammalia alone that are represented in the Hebrew text, as being created in the sixth period; for although our common translation, following the Greek Septuagint, has introduced in that period *creeping thing*, yet this is by no means expressed by the Hebrew term which they have so translated. That term is *remes*, a term which we find applied to the beasts of the forest, as in Psalm civ. 20, 21, " Thou makest darkness, and it is night, wherein all the beasts of the forest *do creep* (tiremos). The young lions do roar after their prey." Instead of *creeping thing*, in the work of the sixth period, our translators would have given a less dubious meaning of the word, and the true sense, indeed, if they had made it *moving* or *walking thing*;—for by *creeping thing* we generally understand the insect tribes. For these, however, the Hebrews have a very different name (sheretz), expressive of their power of rapid oviparous multiplication, as in Levit. xi. 42, "Whatsoever doth multiply feet among all *creeping things*," (hasheretz). The sheretz is expressly named among the beings created, not in the sixth, but in the fifth period of creation in Genesis.

It has been necessary to shew that no creatures but mammalia were created in the sixth period, because Mr Thompson argues that the Iguanodon, with which, as we have seen, he attempts to identify the Behemoth, may have been created at the same time with man; and, in support of his argument, adduces some considerations from the nature of the strata in which the remains of the Iguanodon are found, to shew that, at the time the reptile existed, the earth *may* have been in a fit state for man's habitation. It is unnecessary to argue respecting a mere probability of this having been so, since, both from the history obviously alluded to by Job, and the discoveries of modern geology, we can produce additional proofs that the Iguanodon was not made with man. None of the saurian tribes are named in Genesis as being made with man; but they are expressly named, along with the other ovipara, as being made in the fifth period of creation. Our translators, following here also the Greek Septuagint, have rendered the terms expressing them *great whales*; but erroneously. The original terms are *hathaninim hagadolim*; and, after our inquiry above into the use of the term *Than* or *Thanin*, in the Scriptures, it must be clear to all, that the right translation, in Gen. i. 21, instead of *great whales*, would be *great reptiles*. We may observe, in reference to the plural form being employed in both the noun and adnoun, that by *hathaninim hagadolim* we must understand a plurality of *species* of great reptiles, created in the fifth period; and not one species, which might consist of many individuals. Had the latter been meant, the Hebraism to express it would have been the plural noun with the singular adnoun, as in the passage of Ezekiel already quoted, chap. xxix. 3, where the species of the Nile is expressed, *hathanim* (plural) and hagadol (singular).

We need scarcely remark how admirably this account in Genesis of the period of the creation of the great reptiles is confirmed by the recent discoveries in geology respecting them. Cuvier, in his "Ossemens Fossiles," states as a certain truth, that, at the period of the formation of the strata of Jura, the sea or the land was inhabited by a variety of large reptiles; and Mantel, from his own discoveries, says there was a period when reptiles of appalling magnitude were the lords of the creation. In his "Theory of the Earth," Cuvier says, on the other hand, in respect of the mammalia of the land, that their remains are not found but in strata above the coarse limestone, which is above the chalk; and of man, that his remains have not been found fossil. We are not permitted, then, to suppose, in opposition to both the history to which Job alludes, and these clear discoveries, that the Iguanodon was made with man, and that it was the Behemoth, since no saurian was made with man; nor have the remains of man been found with those of the extinct saurians.

We have shewn, on the contrary, that Behemoth belonged to the mammalia which were made with man; but if we inquire what species of the herbivorous mammalia was meant by the term, we shall arrive, as has been already observed, at no such certain conclusion as those we have reached on other points of our inquiry. To me it appears highly probable, that an animal is meant, different from any that commentators, as far as I know, have yet named; and I will now proceed to state my opinion on the subject, and assign the reasons for it. Let it not be said, that it is presuming too much for any individual to present an opinion different from all those of the learned men who have investigated the subject; for their utter disagreement on the question, even down to the present time, may afford a presumption that no one has as yet furnished the true solution.

It is necessary for forming a right opinion on the subject, that we get at the literal sense of the passage respecting the Behemoth. I conceive that, in some clauses, this is not given accurately in our common translation. Mr Thompson has given a translation by Harris, which is, on the whole, only a paraphrastic one, and therefore lends us no additional aid; and I can perceive no improvement in it, excepting in the first clause of the 23d verse, where, adopting obviously the sense of Bochart, he expresses that which is meant in the original. I set down what I conceive to be a correct translation, and then give briefly my reasons for the more important differences from the common one.

Job xl. v. 15. "Behold now behemoth which I made with thee; he eateth grass as an ox. V. 16, Lo now his strength is in his loins, and his gendering force is in the navel of his belly. V. 17, He moveth his tail like a cedar; the muscles of his terribleness are contorted. V. 18, His bones are as strong pieces of brass; his skeleton is like bars of iron. V. 19, He is the chief of the ways of God: he that made him can press him closely with his sword. V. 20, Surely the mountains bring him forth food, where all the predaceous beasts of strength do contend. V. 21, He lieth under the shady trees, in the covert of the reeds and fens. V. 22, The shady trees cover him with their shadow, the mingled trees of the river-*bank* compass him about. V. 23, Behold the flood may press upon him, he will not fly for fear; he is secure though Jordan rush against his mouth. V. 24, Will any capture him openly, (literally *in*, or *before his eyes*), and bore his nose with a gin?"

The first variation from the common version is in v. 16; in which the word, here translated *gendering force*, is clearly proved by Parkhurst to have this meaning, as in Genesis xlix. 3.; Deut. xxi. 17.; Psalm lxxviii. 51.

V. 17, The term here translated *terribleness*, signifies not only fear, as might be proved by many examples, but also the object of fear, as in Genesis xxxi. 42, 43. The picture in the whole verse is that of an animal tossing its tail, and at the same time putting itself in contorted threatening attitudes, inspiring fear in the beholders.

V. 19, To *press closely*, instead of the common version to *approach unto*, is justified by the obvious meaning of the Hebrew in Genesis xix. 9. In this last passage, our translators have once rendered the word, *stand back*, and once, *pressed sore*. The former translation is an error, arising from the supposition which the lexicographers have adopted, that the assailants addressed Lot. But the result shews they addressed each other. They did not say to Lot "*stand back*," but they said to each other, "*press closely*," and the result followed, "they pressed closely upon the man."

V. 20, *Predaceous beasts of strength*, in place of the common translation, *wild beasts of the field.* Haith is the Hebrew word now translated *predaceous beasts*, which is its proper meaning; as is evident from Leviticus xxvi. 22, and other passages. "I will also send *wild beasts* (haith) among you, which shall rob you of your children, and destroy your cattle, and make you few in number; and your highways shall be desolate." Haith, behemah, and remes, are the beings named in Genesis, as created in the sixth period with man. We perceive that by the two first are meant the larger predaceous animals, and the larger herbivorous animals. Remes, instead of *creeping thing*, obviously expresses the smaller mammalia, as has been already observed.—*Of strength*, in this 20th verse, is expressed in the original, and has not been expressed in the common translation. At the end of the verse, to *contend*, instead of the common translation, to *play*, is justified by 2d Samuel ii. 14, where the *play* is a bloody contention. The young men playing, "caught every one his fellow by the head, and thrust his sword into his fellow's side; so they fell down together."

V. 23, The translation here is literal, and corresponds with that of Bochart.

V. 24. This is also a literal translation, and nearly that in the margin of our common translation.

There are a few other changes justified by the common lexicons, but not worthy of further detailed remark.

It appears to me that every point of the description here, is correctly applicable to some large unreclaimed species of the Bovine genus of Linnæus; and I shall proceed to give a detail of the points of agreement, in the order in which they occur in Job, with reference to the largest unreclaimed species with which we are now acquainted, the Buffaloes of India and Africa.

1*st*, The Buffaloes, as we have seen, are to be included among the animals made in the sixth period of creation with man.

2d, They eat grass as oxen.

3d, The whole bovine genus are full and compact in the region of the loins, when compared with many other races of animals, whose bodies are there relatively slender.

4th, The second character of Behemoth, in the 16th verse, is more obviously seen, at a glance, in the bovine genus than in any other tribe of mammalia.

5th, The tail of this genus, with the bushy tuft of hair at its extremity, and which the animal tosses continually when teased with insects, or threatening combat, would readily suggest, to a poetical and amplifying imagination, the simile of the bare trunk and branchy top of the cedar.

6th, The bovine genus, when threatening combat, place themselves in contorted attitudes, which inspire fear in those who see them.

7th, The bones and skeleton are strong and large, as described by Job, in his poetical language.

8th, To an individual living near the valley of the Jordan, who might be unacquainted with the elephant and larger species of Rhinoceros, one of the large Bovine species, to which I shall afterwards refer, might appear the chief of the works of God, among the land animals; and it is obviously with them that the comparison is made in the first clause of the 19th verse; for the author says nearly the same thing of the Leviathan (which would raise in his mind a different set of comparisons), that he says here of the Behemoth. In Job xli. 33, it is said of the Leviathan, " upon earth there is not his like."

9th, Of some of the large Bovine species to which I shall afterwards refer, it might also be said, by one who knew not the arts of killing it, afterwards invented,—" That he who made it," could alone make a weapon fit to assail it.

10th, The bovine genus are inhabitants of the mountains as well as the plains. I am informed by a gentleman, who was long in India, that wild buffaloes, a race which we shall afterwards see delight also in fens and rivers, are found in the Madras territory, where only he had been, in places remote from rivers and swamps.

11th, Even the tame buffaloes of India set at defiance, and beat off, and kill, the Bengal tiger, one of the largest predaceous animals. Colonel Hamilton Smith says of a caste of herdsmen in India, "they ride on their favourites, (buffaloes), and spend the night with them in the midst of jungles and forests, without fear of wild beasts," (Griffith's Cuvier, vol. iv. p. 394). A combat between a buffalo and a tiger is a common sport at some of the native Indian courts; and such a combat, seen by himself, has been very graphically described by Sir Basil Hall, in his delightful "Fragments;" where the tiger, when brought into the presence of the buffalo, manifested, by his motions of fear, an instinctive feeling, that he was not a match for his adversary.

12*th*, The Buffaloes, while at times they feed on the mountains, delight also in the jungle, and shade of trees, and to wallow in the mud, and even to bury themselves in it up to the eyes.

13*th*, They take the water without fear, and even it appears resort to the streams for pastime or food. Of the Cape buffaloes, Colonel Hamilton Smith says, they "swim with great force," (Griffith's Cuvier, vol. iv. p. 386). He says also of the common arnee of India, "These live gregariously in woody swamps or plains, occasionally floating in whole droves down the Ganges. They are said to plunge under water, and raise aquatic plants with their horn to the surface, where they feed on them, while driving with the stream." (Id. vol. iv. p. 391).

14*th*, The domesticated oxen and buffaloes are secured, in the East, and have probably been so from the earliest times, by a ring or rod passed through the gristle of the nose,—an effective method of guiding the animals, which is becoming common in managing our own bulls and oxen. By the obvious reference to this practice in respect of the domesticated races in Job, we may infer that the Behemoth was of a similar race; though from his size or ferocity, unmanageable by similar means. The case is very different here from what it was in respect of Leviathan, who is represented in Job as not to be captured by a hook; for there the allusion is not to any means of capturing a similar animal, but obviously to the fisherman's hook.

The characters of Behemoth are thus, in every respect, those of the Bovine genus, if we can find among the latter a species of size and ferocity which render it untameable by man. Such a species, in fact, yet exists in the African buffalo, described in such formidable colours by Bruce, Thunberg, and other travellers, and yet unsubdued by the colonists of the Cape, after attempts have been made to turn his great strength and swiftness to account for the labours of man. Colonel Hamilton Smith has made us acquainted also with one, perhaps more than one, species of wild Indian buffalo, that for size and ferocity would answer to the description in Job; for in that description we must always remember, there is much of poetical amplification, like that which we find in the description of the horse, chap. xxxix. 19–25. Colonel Smith refers to a gigantic arnee, found in the eastern provinces and forests at the foot of Himalaya, which the Burmas consider, next to the tiger, the most dangerous and fiercest animal of their forests, (Griffith's Cuvier, vol. iv. p. 389). He refers also to an anecdote of Captain Williamson, where a true Arnee pursued a sportsman to his elephant, and ran its horns under his belly to lift him up. This individual was killed, and was upwards of six feet high at the shoulder, nearly three feet in breadth at the breast, and the horns 5½ feet long. (Id. vol. iv. p. 390.)

The Behemoth was an inhabitant of the valley of the Jordan; and is it not more likely that some large species of the buffalo might, in early ages, have inhabited that limited district, than the elephant or hippopotamus? each of which has been conjectured to be the Behemoth; although both obviously want some of the characters, and appear to delight in more southern regions.

From what we know now of the distribution of animal species, and that some very notable ones are sometimes confined to narrow spaces of the earth, we may be at full liberty to suppose the existence, in early times, of a large Buffalo peculiar to the valleys of Palestine and Syria, surrounded as these nearly are by the sea and sandy deserts The extirpation of such a species, from a narrow country, by the progress of human art and industry, would not be without a well-authenticated parallel in Europe. The Urus has, since the time of Julius Cæsar, been extirpated from Germany; and naturalists are ignorant whether there exists now anywhere the formidable animal which he describes under that name, as being in size little less than the elephant, of the figure of a bull, of great strength and swiftness, so fierce that it spared neither man nor beast on whom it once cast his eyes, untameable even when taken young, and which the resolute German tribes had no means of overcoming, but by capturing it in pitfalls; an art deemed so laudable by them, that they bestowed public honours on those who excelled in it. There are obviously fabulous circumstances in the descriptions of some other animals which Julius Cæsar has transmitted to us, as he himself had not seen them, but had taken the accounts others gave of them; but nothing appears fabulous in his account of the Urus,—on the contrary, the former existence of such a large animal, of the Bovine race, is demonstrated by their being frequently found in England, as well as on the Continent, in peat-mosses, drained lakes and marshes, and the most superficial strata, remarkable skulls of that race, having peculiar specific characters, and about one-third larger than the skulls of domesticated oxen. Cuvier has shewn that these are remains of the Urus of Julius Cæsar, whose existence, in a live state, he has traced to a much later era than his.

The Urus may anciently have existed in many countries besides those in which we have an account of its being an inhabitant. We have no evidence that, like the Indian buffaloes, it sometimes betook itself to the marshes and rivers; but had we that, in addition to its other characters, then it would correspond in all points with the Behemoth in Job.

2d July 1835.

On the Falls of Niagara, and the Reasonings of some Authors respecting them. By HENRY D. ROGERS, F. G. S. of London, &c.

THE magnitude of this noted cataract, the circumstance of its being the outlet for the waters of an immense surface, and the evidences of its retrograde movement towards Lake Erie, have made it a subject of much interest and speculation among geologists. Theories have been framed to explain its origin, and data collected to compute the term of its existence, which, if not always sound, have been, to say the least, curious. Believing that the writers who have conferred upon Niagara its celebrity as a geological wonder, have overlooked some particulars in the surface of the surrounding region, essential to be known in speculating upon its origin and age, I venture to state some views which may, I conceive, tend to inspire salutary doubts in the minds of those who are disposed to theorize upon this difficult subject.

The scenery of this mightiest of waterfalls has been so faithfully and vividly portrayed both by Captain Hall and Mr R. Bakewell junior, that I shall content myself with referring those who have not beheld it to the accounts given by those gentlemen.

The speculations, however, which these and other geologists have entered into, concerning the mode in which the deep perpendicular valley below the Falls was formed, and their calculations of the time employed by the cataract in excavating this ravine of seven miles in length, demand, I think, fresh examination.

I am especially desirous of calling the attention of geologists to the true nature of this remarkable valley below the Falls, as it has recently been much discussed by foreign writers, some of whom are in danger of misconceiving its theoretical bearings, from their imperfect knowledge of the physical structure of the region in which it occurs.

Mr Fairholme, in particular, has indulged in some speculations about the age of the Falls of Niagara, which more precise conceptions of the geology of our lake region, I cannot but believe, would induce him to revise, and perhaps to retract.

All who have investigated Niagara as a geological problem, seem to have assumed it as a conclusion nearly self-evident, that the Falls have necessarily been once at Queenstown ridge, and that they have reached their present place, seven miles off, by virtue solely of their own power of wearing away the rock. This opinion (it is but an opinion) is the result merely of a general contemplation of the scene, and not a deduction from any researches of so rigorous and exact a character as seem requisite to determine such a question. In the present meagre condition of our information respecting the structure of the neighbouring region, such a doctrine cannot, I conceive, be much more than a mere surmise; and I hold it to be altogether premature, to erect upon such grounds any calculation in years of the probable duration of the cataract. The following, in the words of Mr Bakewell, presents the prevailing doctrine regarding the age of the Falls: "On viewing this highly interesting scene, the mind is irresistibly carried back to the time when a mighty flood poured over the once united precipice above Queenstown. This fact cannot be doubted by any one who sees its present appearance, and who duly reflects on what a falling body of water, so immense, so rapid, and so resistless in its course as the river of Niagara, is capable of accomplishing in a series of ages. Taking it for granted that the Falls have been once at the ridge, it is a curious question to inquire, when were they there? An approximate solution to this inquiry will be given, if Mr Forsyth's statement be allowed, of the Falls having receded nearly fifty yards in the last forty years, and if it be granted that this has been the constant ratio of their recession. The distance from the termination of the gorge to the Fall is seven miles, equal to 12,520 yards, which gives 9856 years for the period in which they have been retrograding to where they now are."

Mr Fairholme, proceeding upon nearly the same data,* endeavours to prove that its retrocession was once much more rapid than at the present day, by supposing that the slope of the land gave the Falls originally a less elevation, that the excavation was narrower, and the rocky materials more destructible. In answer to the views of Mr Bakewell, for whose fidelity of description, so far as it goes, I have a high respect, I would suggest that we are hardly entitled to assume the statement of Mr Forsyth as a sufficient basis for a calculation so important in its theoretical applications.

I hold it to be impossible, with the means of measurement heretofore employed, to ascertain, with any approach to precision, the amount of retrograde movement. Does the recession of fifty yards in forty years, asserted by Mr Forsyth, apply to both

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* See London and Edinburgh Philosophical Magazine for July 1834.

Falls, to the whole breadth of the British Fall, or only to that portion of the latter which adjoins Table Rock? To estimate it where the loss of matter is greatest, in the hollow of the Great Horse Shoe, can be done only by means of accurate triangulation, which has not yet been attempted.* To apply the above rate of recession, emanating to four feet per annum, to both Falls, and to Goat Island likewise, as Mr Fairholme does, is, I conceive, an extension of the same error. He imagined that the whole irregular line across both Falls and Goat Island recedes at this rate, though it is manifest that the American Fall is no part of the *receding* cataract. It enters the gorge, *laterally*, having been left by the other Fall at least a *quarter of a mile* in the rear. The true width of the valley at the Falls is therefore no greater than its average width below, as neither the American cataract nor Goat Island contribute to its breadth. In reply to Mr Fairholme's argument, I would remark, that although, in his appendix, he has, on no better information, forsaken the leading feature of his theory, the hypothesis that the ground from the Falls to Queenstown is a regularly descending plane, by which the section of the trough below the Falls would become a triangle, he is still far from giving the ravine its proper profile. The true form of the valley is this. From the Falls to the *abrupt* slope at Queenstown, the land gently declines; but the bed of the river seems to decline equally, falling, in its course of seven miles, 104 feet, and making the perpendicular banks maintain, throughout their whole length, an average elevation of 200 feet.

If Mr Fairholme will reconstruct the section in the appendix to his article, taking care to make it a faithful profile of the surface from the Falls down to Queenstown, giving it the proportions of the base of seven miles to a height of about 300 feet, he will at once discover, that the gorge through which the Niagara flows approaches much nearer to the form of a parallelogram, than to the irregular triangular figure which he supposes. His desire to reduce the solid dimensions of this valley, has led him to represent it with an outline very different from that which belongs to it; and his correction is almost as wide of nature as his first imaginary section.

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* I observe that Mr Conybeare entertains the same doubts of the above rate of movement of the whole Falls. In the Philosophical Magazine for 1831, new series, vol. ix. p. 267, he says, "I must confess my doubts whether the Falls actually do recede, as far as their general line is concerned, at the rate of fifty yards in forty years. I suspect that some partial degradation of the strata has here been mistaken for the general retrogradation." He farther states as his opinion, that Goat Island has occupied, from the period of the earliest account, exactly the same relative position with regard to the Falls that it holds at the present moment.

But suppose, for a moment, that the cataract has been at Queenstown Heights, and of a minor elevation, it by no means follows that the erosion of its bed would on that account be effected more rapidly.

The action of the torrent is not expended in wearing away the whole surface of the wall of rock, but in *undermining* it, by the enormous momentum of the ever-falling mass of waters, which are continually wearing, and removing the loose materials at its base. It is reasonable to infer, that a certain height of fall is necessary to this result.

Having thus shewn the inadequacy of the data upon which the computations of the age of this cataract have been made to rest, I shall proceed to develope some features of the neighbouring district, which render it very doubtful whether the Falls of Niagara ever have been at Queenstown.

It is a very generally received opinion, and may, so far as present evidence extends, be taken for granted, that the country adjacent to Niagara and the lakes was originally covered with a vast lake, or rather inland sea, which some change in the configuration of the region contracted to the still very extensive masses of fresh water now remaining. The passage of such a body of water over the surface would deeply indent all the exposed positions of the land. Rushing, in its descent, from Lake Erie to Lake Ontario, from a higher to a lower plain, and across a slope like that at Queenstown, it would inevitably leave a deep and long ravine. But, further, the whole of this region has been grooved and scarified by the same far-sweeping currents which denuded the entire surface of North America, and strewed its plains and mountains with boulders, gravel, and soil from the north. Such a diluvial valley of greater or less length and depth was, I cannot help believing, probably the commencement of the present remarkable trough below the falls.*

In the present very imperfect state of information respecting the superficial deposits around the shores of our western lakes, it is out of our power to do more than conjecture the geological era of the supposed retreat of the waters, which appear to have covered at one time the vast plains surrounding them. Whether such an event were caused by the general rush of waters from the north, or whether it is to be viewed as having occurred subsequently, we have no means of judging.†

There is this important fact, however, that none of the superior secondary or tertiary formations of our Atlantic coast, have been discovered in the region of these lakes, shewing clearly that all that portion of the continent emerged from beneath the ocean at a very remote period. The drainage of the region has very properly been repeatedly modified since that day, and during some or perhaps several of these changes in its hydrography, Niagara acquired its present remarkable shape.

If any credit be due to the consideration here advanced, it must be obviously improper for geologists to aim at computing the time which the Niagara river has consumed in excavating its way to the spot where it now pours off its waters, since to ascertain what portion of the ravine below the Falls may have resulted from other causes than the cutting power of the stream, is clearly beyond our ability.

It is an observation of Professor Sedgwick, that the existing valleys of any country are generally the result of the joint agency of many causes, and the remarkable valley of the Niagara river, notwithstanding the simplicity of its features, may exemplify this principle.

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* Mr Fairholme, in ascribing to the Niagara the duration which he has, obviously regards it as post-diluvial; he is therefore constrained to grant the pre-existence of the denuding sections for which I contend.

† There are in the class of those who assign to it a greater age, some who would be inclined to consider it as also partly ante-diluvian. To these my argument is similar; to-wit, that it is not credible that any powerful diluvial current should traverse the surface of this region with the deep valley of the Niagara river previously existing, without materially augmenting the length and magnitude of the ravine.

The sides of the Niagara river below the Falls present a narrow belt of table land, extending back a short distance from the verge of the precipice to the foot of a pretty high and steep bank, composed apparently of diluvium.*

These diluvial banks deserve the minutest attention of geologists, inasmuch as they may enable them to detect more clearly than from other data, the former positions of the cataract. It is to be presumed that if they have been washed by the Falls, throughout their whole distance to Queenstown Heights, traces of the fact would be discoverable along their base. I searched in vain a portion of that line for shells, and other river deposits, such as are seen on Goat Island above the Falls. They may, however, exist, and it is important that they should be sought for. These banks are described by Mr Bakewell as curved and water-worn, with large boulders in them, showing, he conceives, that the river once flowed nearly on a level with their summits. But all these appearances are just as indicative of diluvial action, to which, on any hypothesis, the boulders *certainly* belong.

Supposing the existing drainage of this region to have begun immediately after the catastrophe which reduced Lake Erie and its sister lake to their present dimensions, or supposing it to have followed that far mightier event which overspread the whole continent with the debris of its rocks, in either case we are bound to make the Trenton Falls, and the several falls on the Gennessee River, contemporaneous. Now an important question arises here: Are there any facts in relation to the rate of recession of those falls analogous to those of Niagara? None are, I believe, at present known, though these streams admit of a much more exact determination as to changes of position in their falls, than is practicable in the vast and irregular horseshoe fall of the greater cataract of Niagara.

Bearing upon this discussion, there is a still more important question, namely, what features did the surface of the region present after the transient denuding causes above spoken of had ceased, or, in other words, when the existing streams first found their way to the ocean? The surface of the land was evidently what is now denuded and every where scooped into a multitude of valleys, the receptacles, of course, of the newly-formed rivers. This being the case, is it not extremely probable that the depressions into which the Niagara and other rapid streams first fell, were originally valleys of denudation? The existing Falls upon these streams have no doubt contributed, in a considerable degree, to deepen and prolong the gorges through which they flow; but that they *began* these excavations, is what I cannot consider established. One has only to explore the vicinity of Trenton Falls, of the Upper Falls of the Gennessee river at Portage, and of Niagara itself, to perceive that all these interesting scenes are surrounded by the evidences of extensive denudation. The numerous beautiful cascades which distinguish the first-named scene, descend over a series of steps occupying a trough somewhat similar to the one below Niagara, but much deeper and shorter. The total descent here is 300 feet, and the length of the ravine about half a mile, instead of *seven*. There is a striking analogy between the structure of those upon the west Canada Creek, and that of those upon the Gennessee river, and there is this resemblance between them both and those of Niagara, that they all consist of perfectly horizontal strata. The Portage Falls of the Gennessee resemble the Falls of Niagara, moreover, in the great length of the ravine, that of the Gennessee being several miles long, and having also a depth, in many places, of four or five hundred feet.

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* Goat Island is based upon a fine ground homogeneous clay in horizontal stratification, and of a texture so exquisitely comminuted, as to indicate the almost total absence of any currents in the waters from which it was deposited. As one can hardly attribute such deposition to the impetuous current of the present river, it seems very naturally to point to a time when a tranquil lake covered the place of the present rapid Niagara.

Mr Fairholme seems to consider our numerous falls and rapids as the result of what he states to be a characteristic feature of this continent,—the arrangement of the surface in several vast secondary planes, principally composed, as he conceives, of calcareous rocks in horizontal stratification. This is a misapprehension, for it is well known to all who have examined the physical features of the United States, that, in the greater number of our streams, particularly those entering the Atlantic, the rapids are created by the passage of the waters through mountain barriers, and in nearly all cases over rocks highly inclined. This is so, where the Potomac finds a pass through the blue ridge, where the Delaware traverses the same mountain in Pennsylvania, to form the lovely scenery of the Water-Gap, and indeed in twenty other cases which might be cited. An arrangement of our plains in successive plateaus is by no means frequent, and neither Trenton Falls, nor those of the Gennessee, have resulted from any such structure of the surface.

Mr Fairholme has the following passage: " If this point be admitted," (the recession of the Falls), " it is equally obvious that a continuation of the action must occasion a continuance of the effect, and that a time must consequently arrive, when the whole barrier between the lakes must be intersected. This period is, of course, very remote; but it is not the less certain and unavoidable, if the causes now in force continue to exist. The consequences will be most extensive and disastrous, more so, indeed, than any natural event within the range of history. The whole of the upper lakes of North America, which more resemble seas than inland collections of fresh water, will then be lowered by nearly three hundred feet, and the low countries between Lake Ontario and the Atlantic can scarcely escape from being swept at once into the ocean."*

To prove that this is too alarming a picture, it is only necessary to add to the fact, that no barrier, properly speaking, intervenes between the present cataract and Lake Erie, the bottom of which shelves very gently from the shore, and is nowhere deep, averaging only one hundred and twenty feet. The action of discharge must therefore at all times, even long after the cataract shall have *entered the lake*, be extremely gradual, as M. De la Beche has amply shown.† Besides, even supposing it possible for Lake Erie to empty itself thus suddenly, no similar discharge could take place from the other lakes, inasmuch as Lakes Huron and Michigan are cut off from Erie by a river eighty-nine miles in length, and by a difference of elevation of fifty-two and a half feet.

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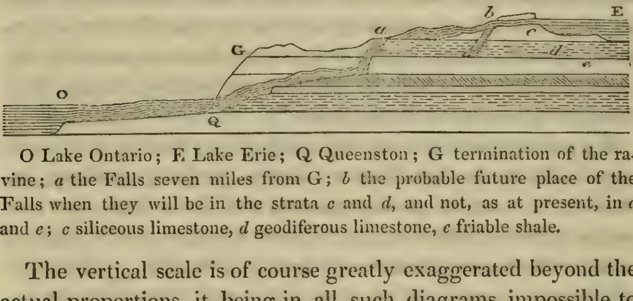
* The distance being twenty-one miles from the present cataract to Lake Erie, and the rate of action being about four feet *per annum*, the time necessary for this great natural operation will be 27,720 years. As the Fall will, however, be higher than it now is when it reaches the top of the rapids, the action cannot be calculated at so much as it now is, and the United States on the coast may therefore safely reckon on a lease of from 30,000 to 40,000 years.—*Fairholme.*

† Third edition of Manual, p. 160.

As to the *future* rate of recession of the Falls, I believe it to be equally difficult to speculate. It is very possible that long before this vast wall of water advances to the brink of Lake Erie, it will have totally altered its shape and aspect. As the Falls retreat, they also *rise*, obeying the general ascent of the land towards Lake Erie. This circumstance, combined with the horizontal position of the rocky beds which they intersect, gradually reduces the thickness of the underlying section of shale, and augments that of the overlying limestone.

The diagram here annexed, will elucidate the arrangement of the several strata along the river, and assist in displaying the future positions which the Falls must occupy as they enter the uppermost beds.



O Lake Ontario; E Lake Erie; Q Queenston; G termination of the ravine; *a* the Falls seven miles from G; *b* the probable future place of the Falls when they will be in the strata *c* and *d*, and not, as at present, in *d* and *e*; *c* siliceous limestone, *d* geodiferous limestone, *e* friable shale.

The vertical scale is of course greatly exaggerated beyond the actual proportions, it being in all such diagrams impossible to represent the distances and the heights in their true ratios.

A still further retrogression will bring the cataract altogether *out* of the inferior shale, the thickness of which at present is ninety feet, and will cause the escarpment of the Falls to consist only of the overlying limestone beds, and ultimately of a still superior stratum, a tough siliceous limestone which occupies the surface from Lake Erie down almost to the Falls. It seems a plausible conjecture, that entering, as it thus certainly must, a new series of beds possessing very different relations of hardness, friability, and thickness, from those which compose the present escarpment, both the *rate* and the *mode* of retrogression will be materially modified. Should the upper stratum, instead of being, as it now is, the *hardest*, become, as it possibly may, before the twenty-one miles are travelled over, the *softest*, there can be little doubt that the present single and majestic fall will divide itself into several cataracts at successive elevations. Niagara will then be almost a counterpart of Trenton Falls, but with far more magnificent dimensions.

In conclusion, it may be well to notice another false impression of Mr Fairholme. He speaks of the fossil remains of the *Elephant* and *Mastodon* of North America being deposited when the waters of Niagara were first set in motion, that is, according to him, when this section had just emerged from the ocean; and he attributes their position, and their shattered state, to the rush of waters simultaneously, by his account, with that emergence. To make, in this manner, those races of animals equally ancient with our coal-fields, may consist with Mr Fairholme's peculiar views of celerity of deposition in strata, but no geologist who examines the features of this continent can acquiesce in such a theory. The diluvial, or more properly *alluvial*, deposites in which such organic remains invariably occur in North America, cover alike all our formations, even the newest tertiary, and are of course separated from the coal formation as to time, by a vast series of intervening periods.

It is therefore quite erroneous to consider as contemporaneous, two events so distinct as the appearance of the coal, and the formation of the diluvium.

About the *shattered* state of these remains, it may be observed, that in the majority of cases, the skeletons of the mastodon have been found remarkably little broken or displaced, sometimes standing *erect*, with all the bones in their natural relations, in the morasses where the animals perished. Now these morasses overlie the true diluvium. In a report on the geology of North America, lately made by the author to the British Association, at its request, an attempt has been made to prove that those races perished on this continent, not by the general deluge, but by catastrophes and accidents of a still *more recent* date.

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Remark by the Editor.—Among the causes that may produce the drainage of the upper lakes, it is obvious, that even a moderate heaving by an earthquake might at once crack the strata that now form the barrier of Lake Erie, and over which the Niagara flows, and thus produce a fissure (not without many examples in other countries), which might, in a few days, or possibly hours, precipitate a large portion of the upper lake upon the nether country, thus producing a debacle of vast sweep and resistless energy, and leaving the present bed of Erie a tertiary valley.

Mr Fairholme, by a strange anachronism, places the coal formation above the chalk, for the sake of bringing it nearer to the deluge. It is difficult to meet a writer, however respectable and intelligent, who takes such liberties with facts—liberties that are equally unwarrantable and useless, for no sudden, short, and violent flood could possibly produce the regular coal-beds with their vast and various stratifications and alternations.—*Silliman's Journal*, January 1835.

Remarks on the Distribution of Insects, and Account of Coleoptera taken in Sutherland in June 1834. By JAMES WILSON, Esq. F. R. S. E., M. W. S., &c. Communicated by the Author.

As the laws which regulate the geographical distribution of insects, and of other organized beings, cannot be accurately determined without the possession of correct local lists of species, I have presumed that an exhibition of the Coleoptera collected in the most north-western portion of Scotland, would prove interesting to those who attend to the study of entomology. So little has been effected in this department in our northern counties, that the English entomologist has as yet scarcely any notion how far the species with which he is elsewhere familiar are extended beyond the shores of the Frith of Forth, or even, in the majority of cases, beyond the latitude of Yorkshire. It is evident, however, that *British* entomology cannot be understood so long as the species either peculiar to, or characteristic of, the northern portions of the kingdom remain unknown; for the subject can never be philosophically considered in its general relations, nor viewed in connection with continental entomology, without more detailed and extensive local data.

In the entire absence, then, of any information regarding the entomological productions of the north of Scotland, I have drawn up the accompanying list of coleopterous insects collected in Sutherland, as an imperfect commencement of those local catalogues of northern species, which, in a completed state, would assuredly tend to illustrate an extremely important department of natural history.

It may be safely presumed (with all due allowance for the indolence of our native collectors) that England is much richer in entomology than Scotland. All countries of which the botanical productions are the most gorgeous and abundant, are likewise the most productive of insect life. Both effects are productive of the same causes,—that is, by a higher and more continuously sustained temperature, a moderate moisture, and a varied soil. So far as the lignivorous species are concerned, the southern parts of the kingdom likewise possess a great advantage over us in the more frequent occurrence of forest trees.

One peculiar feature of the entomology of England consists in several of its rarest species being insects characteristic of the warmer and temperate parts of Europe. Although, on the continent, these extend somewhat further north, owing to the greater heat of summer, the southern portions of our island form their furthest *insular* extent, and they are consequently unknown in Scotland. On the other hand, an equally interesting feature in the entomology of Scotland consists in the appearance for the first time of some of the Scandinavian insects. The greater proportion of our Scottish species occur in Sweden, and it is interesting to know that several of the rarest kinds lately discovered in the northern quarters of the island, such as *Clivina arctica*, *Harpalus micropterus*, *Elaphrus Laponicus*, *Byrrhus æneus*, *Salpingus foveolatus*, and others, are better known in the Scandinavian peninsula than elsewhere, and are indeed characteristic of the north-western division of the continent of Europe. Of these northern kinds many more will doubtless ere long be discovered, and the principal points to which naturalists ought to direct their attention in this department are, the agreement or otherwise of our native species with those of the corresponding latitudes of the continent, and the effects produced on the former by their insular position.

British entomology, when viewed in relation to its general aspect, may therefore be said to exhibit this peculiar feature, that while its warmer portion presents, as it were, the outposts of certain southern species, which entirely disappear as we journey northwards, the northern portion supplies us with a few of the Scandinavian species which do not multiply so freely in more genial climes.

Another characteristic, of more general application both to Britain and Ireland, arises from the circumstance of the British islands lying so much to the west of all those central and northern parts of Europe, with which they might otherwise be compared; while they themselves are flanked in a westerly direction by nothing nearer than Labrador. The consequence is, that many of the central European species do not extend to our meridians, while, as no true insect inhabits the sea, we gain nothing from our vicinity to the vast waters of the Atlantic. Yet several of the diurnal lepidoptera of Britain seem identical with species which occur in North America. This, however, does not in any way arise from our being nearer the western world than are the other parts of Europe, but rather from those particular species being almost cosmopolite, or at least occurring in all the countries of the northern hemisphere. It would, however, be extremely interesting to ascertain whether we have any species altogether peculiar to our cloudy clime. But views of this nature have not as yet greatly exercised the skill of our native entomologists,—perhaps wisely, in as far as a more extensive collection of facts is previously indispensable. The entomology of England, however, is now in a condition to be in some measure legitimately generalized.

- Cicindela campestris.
- Cychrus rostratus.
- Carabus catenulatus.
  - glabratus.
  - clathratus.
  - hortensis.
  - violaceus.
  - cancellatus.
  - arvensis.
- Helobia brevicollis.
  - Gyllenhalii.
- Leistus rufescens.
- Lamprias chlorocephalus.
- Tarus basalis.
- Clivinia fossor.
- Dischirius gibbosus.
- Broscus cephalotes.
- Feronia nigrita.
  - orinomum.
  - nigra.
  - melanaria.
- Abax striola.
- Poecilus cupreus.
- Argutor erythropus.
  - pullus.
- Patrobus rufipes.
- Harpalus æneus.
  - limbatus.

Harpalus ruficornis.
Curtonotus aulicus.
Bradytus apricarius.
Amara eurynota.
familiaris.
communis.
similata.
vulgaris.
Calathus sisteloides.
melanocephalus.
mollis.
piceus.
Olisthopus rotundatus.
Agonum viduum.
parumpunctatum.
mœstum, var. ?
Anchomenus prasinus.
albipes.
Loricera pilicornis.
Badister bi-pustulatus.
Trechus minutus.
Blemus paludosus.
Peryphus littoralis.
Notiophilus biguttatus.
aquaticus.
Elaphrus cupreus.
Blethisa multipunctata.
Dytiscus marginalis.
Hydroporus trivialis.
Colymbetes bi-pustulatus.
uliginosus.
Colymbetes ?
Colymbetes ?
agilis.
Elmis cupreus.
Gyrinus natator.
marinus.
Helophorus aquaticus.
griseus.
granularis.
Hydrobius fuscipes.
melanocephalus.
orbiculosus.
Sphæridium 4-maculatum.
Necrophorus vespillo.
Oiceoptoma sinuata.
rugosa.
thoracica.
Silpha obscura, alpine var. ?
Phosphuga atrata.
Meligethes viridescens.
Byrrhus pilula.
fasciatus.
æneus.
varius.
Hister carbonarius.
Geotrupes stercorarius.
sylvaticus.
lævis.
vernalis.
Onthophilus striatus.
Aphodius fossor.
rufipes.
terrestris.
fimetarius.
Phyllopertha horticola.
Serica brunnea.
Trichius fasciatus.
Cataphagus obscurus.
Ctenicerus pectinicornis.
tessellatus.
cupreus.
Anathrotus ruficaudis.
niger.
Selatosomus æneus.
Elater minutus.
Hypnoidus riparius.
Cataphagus obscurus.
marginatus.
Campylus linearis.
Atopa cervina.
Malthinus biguttulus.
Telephorus rusticus.
dispar.
bicolor.
nigricans.
testaceus.
pallidus.
Anobium castaneum.
Hylobius abietis.
Sitona lineata.
Hypera arator.
Barynotus mercurialis.
obscurus.
Strophosomus coryli.
Phyllobius argentatus.
parvulus.
uniformis.
mali.
mali, var. ?
Thylacites geminatus.
Sciaphilus muricatus.
Otiorhynchus tenebricosus.
lævigatus.
atro-apterus.
Rhagium bifasciatum.
Donacia sericea.
simplex.
cincta.
And several other species.
Galeruca tanaceti.
capreæ.
Luperus fulvipes.
Phædon vitellinæ.
raphani.
Chrysomela fastuosa.
staphylæa.
Coccinella tredecim punctata.
Helops caraboides.
Aleochara concolor ?
Tachyporus chrysomelinus.
Tachinus marginellus.

Tachinus rufipes.
Creophilus maxillosus.
Staphylinus murinus.
castanopterus.
stercorarius.
æneocephalus.
Goerius olens.
Ocypus similis.
Quedius tristis.
Quedius picipennis.
Philonthus politus.
splendens.
varians.
Othius fulgidus.
Gyrohypnus longiceps.
linearis.
Lathrobium lineare.
Stenus ?

A few brief notes may be added to the preceding list. Of the seven Cicindelæ recorded as British, the one first above named, *C. campestris*, is the sole Scottish species. It seems to occur from the Land's End to Caithness. Above two hundred species of the genus are known to naturalists as inhabiting the various regions of the earth. They spread from New Zealand to the northern parts of Siberia, and this very extended distribution makes it the more remarkable that only a single species should be found in Scotland, more especially as six others occur in England.

Of the genus *Carabus*, all the species hitherto recorded as Scotch were found in Sutherland, with the exception of *C. monilis* and *nitens*. At the absence of the latter I was somewhat surprised, as it occurs in several other wild and heathy districts of Scotland; but, as we perambulated the county in so many directions without seeing it, I think it may fairly be inferred to be either entirely absent or extremely rare. *Car. catenulatus*, as might have been expected, was extremely common, and occurred not only on plains, but on the tops of high mountains. *C. violaceus* was much less abundant, and did not occur on the mountains at so high an elevation as the last named. *C. hortensis*, so common in the southern and central districts of Scotland, is extremely rare in Sutherland, and occurs only in the lower grounds. The localities of these three species serve to illustrate in an interesting manner the similarity of effect produced by an elevated position and a high latitude. The two former being found to occur on mountains as well as on plains in the southern parts of the island, extend to the extreme north, and without any great diminution in their numbers; but the last named being naturally more frequent in valleys, and in low lying sheltered situations, seems gradually to decrease in amount as we proceed northwards, and was not observed at all in the district of Cape Wrath. *C. glabratus*, so noted as almost exclusively a mountain species, and so well known in Sweden and Russia, was, as might have been expected, extremely common on all the hills in Sutherland, especially in the neighbourhood of moist pasture,—the rough or rocky moorland being apparently less favourable to its increase. But one of the most interesting features of the county in relation to this genus was the comparative frequency of *C. clathratus*, an insect hitherto regarded as extremely rare in Britain, and scarcely known as an English species. I think that nearly fifty specimens were taken in the course of a few weeks' perambulation, during which, for days at a time, our attention was almost entirely devoted to other objects, and during none of which was entomology an exclusive or engrossing occupation. It may be mentioned, that this fine insect is well known in Sweden, Hungary, and Siberia, and also (a circumstance which renders its rarity in England the more remarkable) that it extends on the continent southwards from France into Italy, where I know it occurs at least as far southwards as the plains of Pisa. Comte Dejean mentions, that such as occur in the more southern parts of Europe are larger, and furnished with membranous wings beneath their elytra; while the northern individuals are of smaller size, and apterous. The specimens collected in Sutherland belong to the latter variety. This range of character in relation to such important parts as the organs of flight, is itself a remarkable circumstance; nor am I aware that any other true Carabus exhibits a variety with membranous wings, the insects of that genus being all described as apterous, that is, possessed of wing-cases alone.

Helobia Gyllenhalii, an insect well known in Lapland and Siberia, is frequent on many of the Scottish mountains, and has been taken on the summit of Benlawers, at an elevation of upwards of 4000 feet. Mr Stephens supposes it to be synonymous with *Helobia nivalis* of Paykul, a name probably bestowed by the Swedish naturalist, in consequence of the snowy heights at which it not unfrequently occurs. It was by no means uncommon in Sutherland.

Leistus rufescens was found to occur in the corners of inclosed pastures, usually beneath collected heaps of stones. In other parts of Scotland it is more frequent under damp moss, on the decayed stumps of trees.

Lamprias chlorocephalus, a very beautiful small insect, not frequent in any part of Scotland, occurred sparingly along the southern borders of Sutherland; but, so far as we could observe, disappeared in the central and northern districts.

Of *Tarus basalis*, a species which, in Sweden, occurs on plains and throughout the central and southern portions of Europe on high mountains, only a single specimen was taken in Sutherland, in a valley of considerable elevation.

Abax striola, rather a rare insect, as far as hitherto observed, in other parts of Scotland, was not unfrequent. *Blemus paludosus* was rare in the north, although the species is known to extend to Shetland. *Blethisa multipunctata*, a very scarce insect in Scotland, occurred to our observation only on one occasion, in a marsh in Cromarty, near the southern border of Sutherland. Two or three were taken together, running rapidly over the moist mud, in company with *Elaphrus cupreus.* A fine variety of this insect (*B. multipunctata*), was lately found by Mr Eschscholtz, in Kamtschatka. On the European continent it extends as far north as Lapland.

We looked anxiously, but in vain, for *Elaphrus splendidus*, an insect lately taken, for the first time in Britain, by Mr Lyell, younger of Kinordy, on Cat-Law, Forfarshire. As it is common in Lapland, we deemed its occurrence in Sutherland probable, more especially after its frequent capture in a more southern county.

Among the aquatic beetles, I regret to say we did very little, our time, when tempted by the presence of the " pure element of waters," being almost exclusively occupied in angling,--an amusement extremely agreeable in the morning, and (when successfully followed) remarkably useful in the afternoon.

Silpha obscura appears in Sutherland under an unusual aspect, being frequently of a reddish-brown colour, instead of dull black. I at first thought that this peculiarity of hue might be the result of immaturity, as many coleopterous insects are pale of colour, on their first assumption of the perfect state; but its general prevalence and continuance afterwards induced me rather to regard it as an alpine variety, the result of some peculiarity of climate.

Of *Byrrhus æneus* five specimens were taken beneath some flat stones, on the southern descent to the Ferry of Kylestroem. This species has been recently added to the British Fauna, and was first taken near Newbattle House, in the county of Mid-Lothian. We do not know whether it has yet been found in England. It is well known in the north of Europe.

Of the Geotrupidæ, *Geotrupes vernalis* and *lævis*, both of which are scarce species in most parts of Scotland, occurred rather frequently. *G. stercorarius*, which, throughout Britain, is the most abundant, was comparatively scarce in Sutherland, from which it may perhaps be presumed that its centre of dominion is to the south of Scotland. *Phyllopertha horticola*, which occurred in great profusion on sandy pastures near Keoldale, and was not seen elsewhere in the county, exhibited a peculiar variety of a greenish-black colour.

Of *Trichius fasciatus*, an extremely rare insect, which, on the Continent, is known to extend from the south of Russia, through Siberia, and into Kamtschatka, a single specimen only was found on the north bank of the Oikel.

Ctenicerus tessellatus, one of the Elateridæ, distinguished by its pale scutellum, had not previously been found in Scotland, and another of the same family, *Campylus linearis*, was new to the northern parts of the country.

Of the numerous and difficult family of the Curculionidæ, I shall here mention only *Otiorhynchus lævigatus*, as an insect which occurs occasionally at great heights. In Sutherland we did not observe it, except on the sides of hills of no great elevation; but, on another occasion, it was captured on the summit of Ben-na-mac-dhui, the loftiest mountain in Scotland, at an elevation of 4300 feet.

Of the *Longicornes*, as might have been expected from the almost total want of wood in Sutherland, the number noticed was extremely small. Indeed *Rhagium bifasciatum* afforded the sole example of the tribe.

Of the genus *Donacia* several species were met with, not mentioned in the preceding catalogue. They presented some variations in the specific characters which have hitherto prevented their being determinately ascertained. Of the more limited genus *Helops*, every where rare in Scotland, the species become very scarce in the north. Only two specimens of *Helops caraboides* were taken in Sutherland, by Mr Selby. Of the *Chrysomelidæ* very few fell under our observation; and it may be noted as a singular feature in the entomology of this north-west district of Scotland, that not a single species of the genus *Coccinella*, usually so frequent in other parts of the country, was observed. The only insects of the genus captured during the excursion were three specimens of *Coccinella tredecim punctata*, which were taken by Dr Greville, not in Sutherland, but in the adjoining county of Cromarty, between Invergordon and Tain.

On the Chemical Nature of Fossil Scales, as illustrative of the Character of the Animals to which they have belonged. By A. Connell, Esq. F. R. S. E. &c.* Communicated by the Author.

The difficulty which has often been experienced in determining merely from external characters, whether fossil scales belonged to a saurian animal or a fish, and the importance of this question, sometimes as characterising the geological formation, and at others as fixing the epoch of the fossil animal, render the inquiry of some interest, whether chemistry can lend its aid in solving the problem.

It was, I believe, Mr Hatchet, who first drew a distinction, in point of chemical nature, between the scales of recent reptiles and those of recent fishes. He ascertained that the scales of serpents, lizards, and such like animals, consist chiefly of a horny substance, of the nature of coagulated albumen; whilst those of fishes he found to contain a considerable proportion of phosphate of lime, and to be of the nature of bone †. Such experiments as have since been made, are confirmatory to a great extent of these views. Chevreul found the scales of various recent osseous fishes to contain large quantities of phosphate and carbonate of lime, and to be very analogous to bone. I have made the same observation in regard to cartilaginous fishes, having obtained from the dorsal scale of a sturgeon (Acipenser Sturio), 53.85 per cent. of bone-earth. * I have also examined the scales of one or two recent crocodiles, principally with the view of ascertaining what proportion of indestructible earthy matter entered into their constitution, and have found that its amount was very trifling. A scale from the thorax of a Nile crocodile about eight feet long left, when completely incinerated, little more than one per cent. of incombustible matter, which consisted of phosphate and carbonate of lime. The carinated dorsal scales of crocodiles appear to contain a rather larger proportion of bone earths. In one of them, taken from the back of a small Nile crocodile, four feet long, I found that a kind of bony axis, consisting principally of phosphate, and carbonate of lime, traversed the scale longitudinally; but still the proportion of earthy matter in the whole scale, including this bony axis, did not much exceed 3 per cent. From the general appearance of the carinated scales of larger crocodiles, it seems likely that the proportion will be found to be larger; but I have not had any opportunity of examining them. The flat scales of crocodiles, however, such as those on the sides and belly, there seems no reason to doubt, consist almost entirely of perishable animal matter, with only such a minute portion of bone-earth as above stated; and therefore differ essentially from the flat scales of recent fish, which contain about half their weight of bone-earth, and may be considered as true bone.

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* Read to the Chemical Section of the British Association, at Dublin, 12th August 1835.

† *Phil. Trans.* 1800, p. 373–4.

It is evident that these considerations afford a clew to the solution of the proposed question. When a fish becomes fossilised, the bone-earth of its scales will survive like that of its other bones, whilst the other animal matter entering into their consti-

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* It thus appears, that although the internal skeleton of a fish may be entirely cartilaginous, the same animal may nevertheless have a portion of its external covering composed of true bone; a circumstance which seems to increase the probability, that the fossil Gyracanthus of Burdiehouse, although possessed of external bony fin rays, may have been a cartilaginous fish. See the Memoirs, *Edin. Trans.* vol. xiii.

tution will gradually decay, and if water holding calcareous or siliceous matter in solution, has access to them, the place of the perishing animal matter will probably be taken, to a greater or less extent, by the dissolved mineral matter. The fossil scale will thus consist, when the process of mineralization has been completed, of the original animal earths, and of the substituted mineral matter. On the other hand, if a saurian animal were mineralized, the perishable animal matter, of the nature of coagulated albumen, forming nearly the whole mass of its scales, at least of such as are flat, and belong to the sides and belly, would gradually be dissipated; and if no mineral matter had been presented to them during this process of decay, in such a state as to be fit for substitution, no vestige of the scale will be left; but if such mineral matter has been present, it will probably gradually take the place of the decaying animal matter, and we shall have a substance preserved, possessing the form and even structure of the original scale, but containing little or no bone-earth, because none existed in the original object, and consisting entirely, or nearly so, of the replacing substance, whether siliceous or calcareous,—just as we find perishable vegetable objects preserved in a mineral form in all the delicacy of their original structure, although no longer presenting even a trace of combustible matter. Assuming, therefore, the scales of extinct saurians to have had the same composition as those of living reptiles, we shall expect to find true saurian scales, where such exist, to contain little or no bone-earth, and to consist almost entirely of the mineralizing substance.

These principles would, I believe, have been sufficient to decide the disputed question as to the nature of the organic remains found in the limestone of Burdiehouse, had not that point been previously settled on other grounds. The analysis of the scales of that deposit has been already published in the Transactions of the Royal Society of Edinburgh. Not only was it found that they contained a large proportion of bone-earth, but it was also ascertained that the proportions of the constituents bore a very strong analogy to those of the scales of a recent fish, supposed by M. Agassiz to be the living type of the Megalichthys of Burdiehouse. The siliceous matter in the fossil scales had apparently taken the place of the perishable animal matter which had existed in those scales while the animal was alive; as was shewn by the circumstance, that when the fossil scales were acted on by acids, the siliceous matter remained undissolved in the form of a fine siliceous skeleton. In order to connect these analyses with those which are to follow, I again subjoin the results. In the scales of the recent Lepisosteus, Chevreul found

Phosphate of lime, 46.20; carbonate of lime, 10.; gelatinous animal matter, 41.10; phosphate of magnesia, 2.2; fatty matter, 0.10; carbonate of soda, 0.10; = 100.

Those of the Megalichthys of Burdiehouse I found to contain,

Phosphate of lime, with a little fluoride of calcium, 50.94; carbonate of lime, 11.91; hydrated siliceous matter, 36.58; potash and soda, .47; bituminous matter, .12; phosphate of magnesia, trace; animal matter, trace: = 100.12.

And these results suggested the following comparison:—

			Scales Lepisosteus	Scales Megalichthys.
Phosphate of lime,	-	-	46.20	50.94
Carbonate of Lime,	-	-	10.	11.91
Animal matter replaced in the Megalichthys, by hydrated siliceous matter,			41.10	36.58
			97.3	99.43

Little doubt could therefore remain as to the true nature of the fossil scales of Burdiehouse.

I am indebted to Lord Greenock for specimens of fossil scales from two other localities, of which I now propose to state the analysis.

The first of these are the fossil scales found by his Lordship imbedded in the coal of Craighall, near Edinburgh. These have been considered to belong to the same fish as those of Burdiehouse, and the chemical analysis fully supports this view, the principal difference being that replacement of the animal matter has not occurred to so large an extent as in the Burdiehouse scales, a fact for which we might have been prepared by the circumstance, that their lustre and general state of preservation is not quite so fine as in the latter instance, although still wonderfully perfect.

The portion of the Craighall scale analyzed, was nearly 1 inch long by half an inch broad, and was evidently only a fragment of a still larger scale. It adhered to coal, and had a yellowish colour, and a good deal of lustre, but the surface was not observed to be punctured.

The chemical details of analysis need not be entered into, as the general process followed was the same as that formerly given in the Edinburgh Transactions.

The result was as follows:—

> Phosphate of lime, with a little fluoride of calcium, 55.75; carbonate of lime 15.86; siliceous matter, 16.17; alumina, 2.82; phosphate of magnesia, trace; potash, and soda, partly as sulphates, 1.06; water and bituminous matter, 6.46 = 98.12.

If we suppose that about one-half of the animal matter in the original scale had been replaced, partly by siliceous and partly by calcareous matter, its composition must have been entirely analogous to that of the scales of the Lepisosteus and Burdiehouse fish.

The other fossil scale analyzed was a very beautiful one from the Tilgate Forest, Sussex. Its form was quadrangular, without those bifurcating processes which Mr Mantell has supposed to be characteristic of fish-scales, and without any carinated appearance. Size about three-fourths of an inch square; colour black; lustre considerable; punctures scarcely visible on its surface, with a double lens; thickness varying from one-fifth to one-tenth of an inch. Matrix, a calcareous grit, nearly entirely soluble in acids. This fossil had been considered as the scale of a saurian animal, and I therefore undertook its analysis with some degree of interest, for the purpose of ascertaining how far this view of its origin would be borne out by its chemical nature. The result leaves, I imagine, little doubt that it is to be considered as the scale of a fish.

Its constituents were found to be,

> Phosphate of lime, with a little fluoride of calcium, 60.13; carbonate of lime, 27.94; siliceous matter, 3.42; alumina, .82; phosphate of magnesia, trace; potash and soda, partly as sulphates, 1.43; water, carbon, and sulphur, 6.71; = 100.45.

The large proportion of phosphate of lime here indicated, appears to be incompatible with the idea of this scale having belonged to a saurian animal; and there seems reason to believe that its original composition, as a fish scale, was analogous to that of the others which have been examined. The replacement of the animal matter has evidently been made chiefly by carbonate of lime, of which the matrix consists; and if we suppose that a little less than one-half of the original animal matter has given place to calcareous and a small quantity of siliceous matter, whilst the larger proportion has been dissipated without substitution, the scale in its original state would have a constitution very analogous to the others; the apparently larger proportion of phosphate of lime being in this way accounted for. To what extent the analogy may have gone in other respects between the Megalichthys of the carboniferous strata of Burdiehouse, and the fish of the far more recent beds of Tilgate, is a point which must be left to the decision of those better qualified for the task, upon a due consideration of all the accompanying fossils.

There seems every reason to believe that the occurrence of true scales of saurian animals, in a fossil state, is very rare, compared to that of fish scales,—a fact which the chemical nature of the former fully prepares us to anticipate; and I have never had any opportunity of examining, or indeed of even seeing, an undoubted saurian fossil scale. In the Ossemens Fossiles of Cuvier, I observe only two instances of such remains, notwithstanding the numerous fossil animals of this kind described in that work. One of these occurs in a fine calcareous slab containing vertebræ and ribs of the fossil Gavial of Caen, in Normandy, in which several rows of scales occur in the precise relative position which they must have occupied in life *, and as to which, therefore, not a doubt can exist that they are of true saurian origin. The account given of them by Cuvier is, that "they differ from those of living crocodiles more than any other part of the skeleton, and this crocodile of Caen was undoubtedly the best mailed of the whole genus. The scales are very thick, rectangular, thinner towards the edge, and their whole external surface is hollowed into little hemispherical cavities, of the size of a lentil or pea, and pressed against one another." If the original chemical composition of these scales was the same as those of living reptiles, which there is every reason to anticipate, they ought to contain very little phosphate of lime, and will in all likelihood be found now to consist prin-

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* Oss. Fos. tom. v. part 2, p. 139. Pl. vii. fig. 14.

cipally of calcareous matter, the matrix being of that nature, although siliceous matter may also possibly enter, to a greater or less extent, into their composition. It would be a matter of interest if the friends of science in France would have this matter ascertained.

The other instance mentioned by Cuvier,* refers to fossil scales supposed to belong to crocodiles found in the neighbourhood of Argenton, in the Paris basin. They have a considerable resemblance to those of Caen ; and it is remarkable that they are described as having a very prominent bony crest, " une crete osseuse tres saillante,"—a character apparently having some connection with the circumstances formerly stated as to carinated scales. These fossil scales would also be well deserving of an examination.

I could have wished to have made my examination of such relics more extensive, but in this place no other materials were to be met with.

EDINBURGH, 3*d July* 1835.

Notice of an arrangement for the measurement of the Angles of very Minute Crystals. By the Rev. EDWARD Craig, A.M., Oxon., F. R. S. E, Councillor of the Society of Arts.†

IN the winter of 1833 I was led to investigate the phenomena of chemical action under the microscope, and submitted to the consideration of the Royal Society of Edinburgh, during that session, a short paper on the subject. The method which I adopted was, to place very minute portions of chemical agents upon thin plates of flat glass, and to bring them together ; then, when one plate was pressed down upon the other with a uniform, or nearly uniform, pressure, the substances under examination were reduced to a very thin film, capable of very distinct observation, by transmitted light, and of being easily brought under the focus of a highly magnifying lens.

In this way an endless variety of operations were observed, especially those connected with the application of galvanism to chemical agents. My attention, however, was much directed to the beautifully minute crystallizations which took place in that apparently limited space; sometimes above a hundred exquisitely symmetrical and perfect crystals lying together, and capable of distinct observation, in the space of the 5000dth of a square inch.

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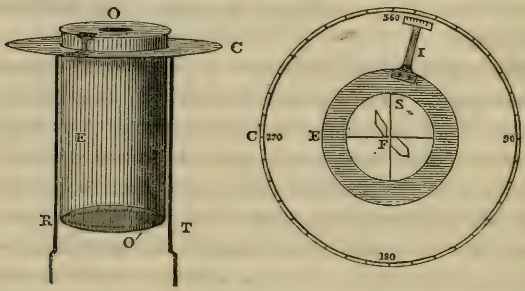
* Oss. Fos. tom. v. part 2. p. 168.

† Received 8th April, and read and exhibited to the Society of Arts for Scotland 20th May 1835.

A difficulty, however, arose as to the nature and composition of these crystals. The substances brought together in this way necessarily meet each other in unmeasured and, to us, indefinite quantities. The whole quantity used was too small for weight; and, even if each substance could have been weighed, when spread with extreme tenuity over a comparatively large surface of glass, it was impossible to ascertain the proportion, or the proportional strength in which one substance would meet others at any given point of the whole surface covered. When changes took place, and new combinations and crystallizations appeared, a degree of doubt therefore arose as to the precise nature of the substance. At all events, the nature of the substance could not be predicated so decidedly as in a case where the whole measured quantity of two or three known agents, known also in their relations, assumed in combination one marked and familiar character. And if this doubt would arise with respect to minute crystallizations originating in the combination of known substances, it would be still more the case in the attempt to analyze by this mode of microscopic investigation some small portion of a substance whose composition was not known; and in which the varied results under the tentative process must be greatly, at least for a time, matter of conjecture.

I have no doubt, after lengthened observation, that familiarity with microscopic crystalline forms, and the accurate recording and classifying of phenomena, would go far to remove much of this uncertainty; but besides this, it appeared to me that, as in the case of many substances, the angles of crystallization are accurately known, the range of doubt might be considerably limited, if the angles of crystals so formed could be submitted to accurate measurement. And, with this view, I adopted the following arrangement for the application of a micrometrical goniometer to the microscope:

The eye-piece of my microscope is a tube about an inch and a half long, having one lens at each extremity; and the tube is smoothly and accurately fitted, not to screw, but to slip down into the tube of the object-glasses, Fig. 1.



The lower lens O′ receives the image from the object-glasses, and this image is examined by means of the upper lens O. In the focus of the upper lens O, and lying directly on the plane side of the lower lens O′, which is plano-convex, I placed two fine silk threads, crossing each other at right angles. The tube of the eye-piece is slipped down, not screwed down, to its right position in the tube of the object-glasses. It can be turned, therefore, either way freely, without affecting the focal arrangement of the microscope, which is regulated by other means. And, by turning the eye-piece round, these fine threads, unmagnified, may be applied to the side of any angle of a highly magnified crystal lying on the field below.

A brass circle C, about two inches and a half in diameter, with a divided scale on its outer edge, was made to screw fast on to the object-tube T, before the insertion of the eye-piece. And to the edge of the eye-piece a small index I was fixed, so as to lie close on the circle C, and point to the graduated division. This index is not a simple point, but a vernier, for indicating minutes.

To measure the angle of a crystal F, it is necessary to bring the point of the angle to the centre of the field of view, where the silk diameters cross each other; then, by turning the eye-piece round, to bring one of these silk lines to coincide with one side of the angle to be measured, and notice the point of the

BRITISH MUSEUM
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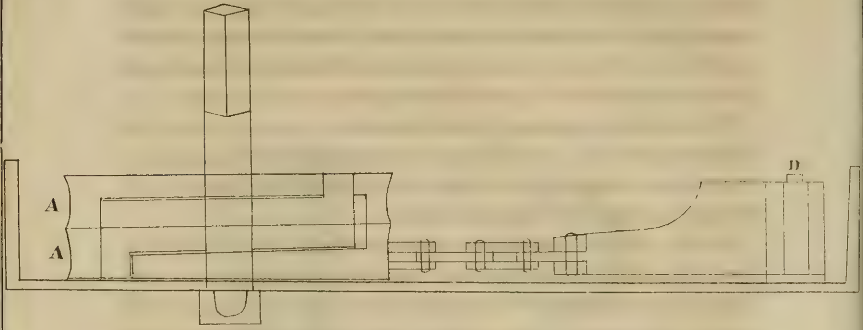
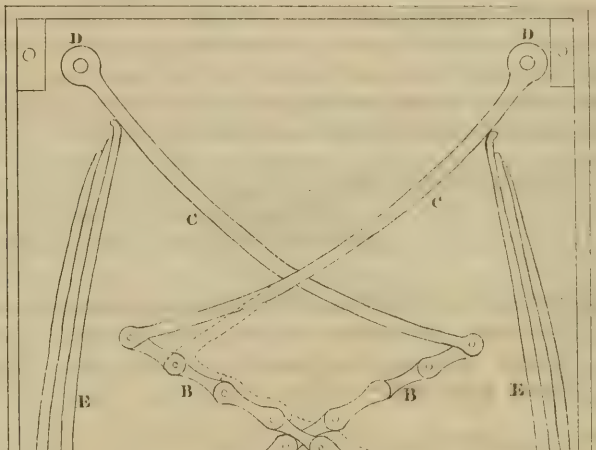
PLATE V.

Edin. New Phil. Journ. Vol. XIX. p. 309.

BEATTIE'S DOOR SPRING.





Forrester & Nichol lithog. Edin.r

scale at which the index stands. The eye-piece must be again turned till that thread has passed over the whole angle, and coincides with the other side of it. The difference of position of the index read off will give the measurement of the angle.

The operation requires some neatness of manipulation, but this is acquired by practice; and if the operation be repeated and a mean taken of the errors which occur each time, it will be easy to come very near the truth.

In this way the angles of crystals not more than the four or five millionth of an inch in size may be accurately ascertained. And the introduction of measurement in such cases has surely a great advantage in this respect, that such crystals are more perfect and sharp than almost any that can be obtained of a large size; and that frequently a whole family of crystals, in all their correlative forms and varieties, will be found lying together; such as, in any size adequate for manipulation, the diligent search of a whole lifetime could not have collected.

EDINBURGH, *8th April* 1835.

Description of a New Spring for Shutting a Door, which goes both ways. By ALEXANDER BEATTIE, Foreman to Mr ROBERT RITCHIE, Ironmonger to the King, 241. High Street, Edinburgh.* With a Plate.

THE spring is contained in an iron-box, 11 inches by 7, and 1½ inches deep, and consists of two horizontal wheels, marked on the section AA, Plate V., lying close above, and parallel to, one another, moving on the same axis with the door, to which the chains BB are attached, and moves with the wheels; the other ends of these chains are fixed to the levers CC, which move on the pivots DD, close to which the triple springs, EE, are made to press upon these levers.

When the lever or door is pressed or opened to the right hand, the under wheel is forced round, drawing back the lever C by means of the chain B, which lever the spring E presses against. There is a cog or stop F on the under wheel, which moves in a groove or slit, cut in the upper wheel, to prevent its being forced too far round.

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* Read to the Society of Arts for Scotland 11th December 1833.

In the same manner, when the lever or door is opened to the left, the upper wheel A is forced round, until it is stopped by the cog F on the under wheel, which pulls round its chain and lever, and is pressed back again by the opposite spring E.

In this manner the wheels change the action upon the springs by the door being moved to the right or left.

The object of the inventor was to make this spring go into less room and at less expense than the one at present in general use. It has also the advantage of being less liable to go out of order, and can be made to turn more than the quarter circle, which the other cannot.

The box may be filled with oil, or the working points oiled occasionally, by removing part of the cover.

N. B.—By enlarging the diameter of the wheels, and strengthening the other parts in proportion, the spring can be made to suit any size or weight of doors.

EDINBURGH, 11*th December* 1833.

Report of Committee of Society of Arts on Mr Beattie's Spring-Door, read 9th April 1834.

Immediately subsequent to the meeting at which Mr Beattie's door-spring was exhibited, it was attached to a swing-door, in the premises of one of the reporters, who has paid constant attention to its action, and who, after a trial of three months, is satisfied that, to its other good qualities, it adds that of durability.

Your Committee are of opinion that the construction of this door-spring possesses several important advantages over any other they are acquainted with; and as, after a trial of sufficient duration, to have shewn latent defects if it had any which had escaped their notice, they see no reason to alter the favourable opinion they had formed of it, they humbly propose that Mr

Beattie should receive some mark of the Society's approbation.*

Your Committee beg to suggest that a small alteration should be made in the form of the pieces on which the chains are wound up, which they think should be so proportioned that, as the resistance of the spring increases, the radius to which the chain applies should decrease, and in this way make the resistance of the door equal in every position, instead of, as at present, being greatest when the door is wide open.

John Robison, *Convener.*
George Angus.

On the different Chronology of the Deluge, according to the Hebrew Text and the Septuagint Translation. In a letter to Professor Jameson, by Mr James Laurie, Edinburgh.

(To the Editor of the Edinburgh Philosophical Journal.)

In your last Number, there is an article by Dr Von Schubert, "On the period of the Deluges of Deucalion, Ogyges, and Noah;" wherein the author commits a very palpable error, which, I think, ought to be pointed out. "The calculation of time," says the Doctor, "in the original passage of the Bible, fixes the Deluge in the year 1656, and the Greek translation in the year 2242, after the creation. These two apparently widely different statements agree more nearly than we should at first sight believe. The Septuagint employed a mode of calculating time according to years of 272 days; consequently, to years consisting of ten months, 2242 years of ten months would correspond nearly to 1656 solar years."

Now, this statement of Dr Von Schubert proceeds evidently upon the assumption, that the Bible announces in express terms the period of the Deluge. Every reader of the Bible knows that it does no such thing; the dates of the Creation and the Flood being left to be ascertained by a calculation of patriarchal generations, as in the following tables. The first column of each table shews the ages of the antediluvian patriarchs at the births of their eldest sons, and the sum of this column gives the year of the Flood. The second column shews the surplus of their years; and the third shews how old they were when they died. In Noah's case, the number is the year of his age when the Flood came.

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* The Society's Silver Medal (value Five Sovereigns) was awarded 12th August 1835.

	I. Hebrew Text.			II. Septuagint.		
Adam, . . .	130	800	930	230	700	930
Seth, . . .	105	807	912	205	707	912
Enos, . . .	90	815	905	190	715	905
Cainan, . .	70	840	910	170	740	910
Mahalaleel, .	65	830	895	165	730	895
Jared, . .	162	800	962	162	800	962
Enoch, . .	65	300	365	165	200	365
Methuselah, .	187	782	969	167	802	969
Lamech, . .	182	595	777	188	565	753
Noah, . . .	600			600		
Total, .	1656			2242		

A single glance at these tables is enough to prove the Doctor's hypothesis to be groundless; for though the sums-total of generations may bear the ratios of ten and twelve respectively, yet the separate sums cannot be brought under such a rule. For instance, Adam's age, according to the Greek text, at the birth of Seth, was 230 years, which, if only of ten months each, would make 191 solar years, instead of the 130 of the Hebrew Bible; and so with the others. The last and largest item of both tables finally disposes of the Doctor's hypothesis; for in both of them Noah's age at the coming of the Flood is the same. What, then, becomes of his ratio here? The agreement, besides, of the two tables in the total age of each patriarch, shews beyond dispute, that the years employed in both texts were of the same precise length, and that their different chronology of the Flood is occasioned by their different statements of the ages of the patriarchs at the births of their eldest sons.

A like difference between the Hebrew and the Greek Bibles extends through the generations of the postdiluvian patriarchs: the calculation from the Septuagint making the date of the Flood 3155, and from the Hebrew Bible only 2348, years before the Christian era.

The real cause of these differences is unknown. The only rational supposition (and which, moreover, is supported by strong circumstantial evidence), which I have heard of, is, that the Hebrew text of the patriarchal ages was falsified by the Jewish doctors of Tiberias, some time in the beginning of the second century of the Christian era, for the purpose of defeating what was, in that age, considered a very strong evidence that Jesus of Nazareth was the true Messiah, viz. that he had appeared towards the end of the sixth millenium of the world's age. This they effected by the very simple process of transferring 100 years from the age of the patriarchs at the births of their sons to the surplus, and by thus shortening the chronology, they thought to bring the world back to its fourth millenium, and thereby defeat the Christian argument. Previous to this time the Septuagint was considered by the Jews as holy and inspired, but afterwards they solemnly cursed it, instituted a penitential fast to deprecate God's wrath against the impious work of the translation, and set about a new translation of their own, conforming to the altered Hebrew text. The Septuagint, however, is supported by the writings of Josephus, the Jewish historian, who professes to have derived his information from the Hebrew sacred books as they existed in his time. It also agrees with the genealogy of Christ given by St Luke, and is altogether more consistent with itself and with profane history than is the short chronology of the Hebrew. Accordingly, chronologists are now pretty well agreed in preferring the authority of the Septuagint, thus making the age of the world at the epoch of Noah's Flood 2242 years, instead of 1656, the date of Archbishop Usher's chronology, founded upon the Hebrew text.

The principles of the *long chronology* are detailed in Dr Hailes's " Analysis of Ancient History." His dates are adopted by the Rev. G. R. Gleig, in his popular " History of the Bible," in preference to Usher's. The modern Jews differ from both, and make the year of the Creation 3760, and of the Flood 2104, before Christ.

Dr Von Schubert also refers to the Samaritan Pentateuch, which differs from both the Hebrew and the Greek. All, however, that need be said of it is, that in it, as in the others, the chronology is deduced from the patriarchal generations, to which his ratio does not apply.

EDINBURGH, 31*st July* 1835.

Notice of the Remains of a Fish found connected with a Bed of Coal at Clackmannan. In a Letter to Professor JAMESON. (With a Plate.)

MY DEAR SIR,

AGREEABLY to your request, I have sent you the accompanying sketches of an Ichthyolite which was found a few years ago in the course of the mining operations of the Devon Iron Company, and in the neighbourhood of their works. Its preservation is owing to the considerate attention of Mr James Wilson junior, the mining-engineer of the establishment, who obligingly presented the specimen to me. From the same intelligent observer, may be confidently expected other valuable contributions to a knowledge of the organic remains to be found in the Clackmannan coal-field.

When this organism was first exhibited to me, I was at no loss to recognise the resemblance between the plates or scales with which it was invested, and which occur in natural juxtaposition, and objects of a similar form and structure, though detached, or unconnected, which, twenty years before, I had procured in the county of Fife, from a bed, covering the Marine or Mountain Limestone on which the coal-formation of that district rests as its fundamental rock. As the consideration of the form, structure, and composition of the organisms from Fife had induced me to consider them as *the scales of a fish*, I was led, under the influence of this opinion, and observing the scales of the Clackmannan petrifaction occupying both sides of the specimen, to seek for traces of the appearance of the vertebral column, and I soon satisfied myself as to the indications of its existence at both extremities of the mass. In this conviction, I despatched the example to Edinburgh, with directions to the lapidary for making a section confirmatory of the views of its nature which I entertained. When in the hands of the lapidary, it was inspected by several members of the Royal Society of Edinburgh, whose zeal in the study of organic remains had received a fresh impulse from the numerous specimens which had been found in the limestone of Burdiehouse. In the opinion of more than one member of the Society, labouring at the time under *saurian* or *sauroid* prejudices, my specimen was pronounced to be the fragment of a reptile, not of a fish.

Fig. 1.

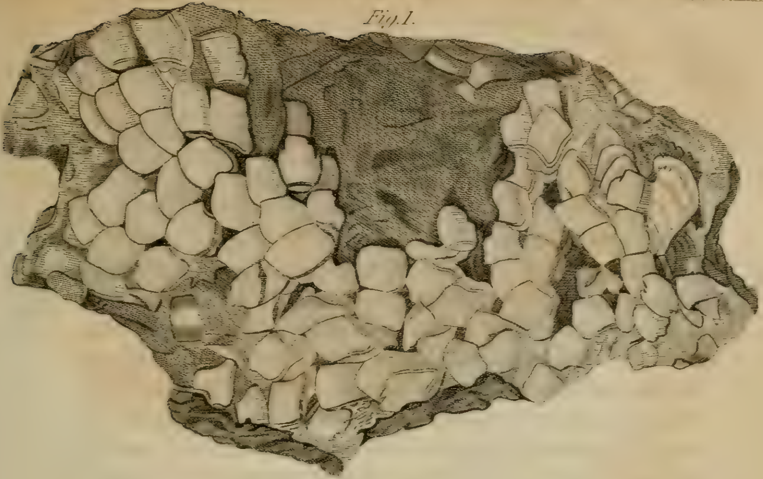


Fig. 2.

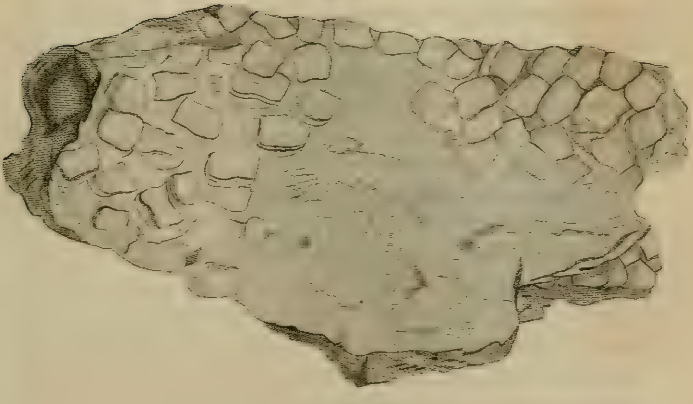
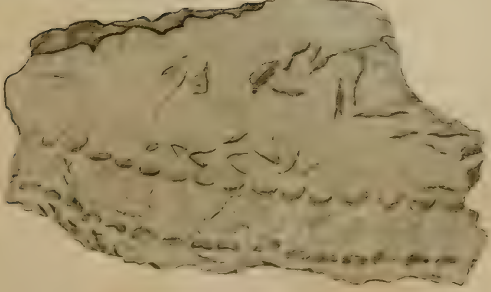


Fig. 3.



BRITISH MUSEUM NATURAL HISTORY.

The organism, of which views of the sides are given in Plate IV. figs. 1 & 2, is upwards of nine inches in length and five inches in breadth at the widest part, and where thickest not exceeding two inches and a half. It is unfortunately but a fragment, as neither head nor tail were found or observed at the time. The dorsal and ventral ridges are incomplete, and there are no traces of fins.

The scales have nearly all the same shape, whether they occupy a dorsal or a more ventral position. The rhombic form generally prevails, especially among those which preserve their relative position, and have been least acted upon by compression. The exposed part of the scale is smooth, glossy, and minutely punctate. Another portion, and probably that which has been covered by the imbrication of the neighbouring scales, has a dull and slightly rough surface, with an intervening groove at its junction with the glossy or exposed part. This appendage to the scale occupies an angle and two contiguous sides, and gives a marked character to the organism, when found detached.

The section of the specimen brought into view, as I had anticipated, the vertebral column, extending through its whole length, eighteen divisions of which can be readily traced. The vertebræ, where they do not appear to be compressed, are about an inch and a quarter in diameter, and half an inch in length, and of the form expressed in the sketch, Fig. 3, (where the engraving has been inadvertently reversed, so that the *section* of the upper part of Figs. 1. and 2. here appears on the lower side of the figure). They exhibit the appearance of rings, the cellular bony matter remaining, to mark the peripheral substance of the vertebra, while but indistinct traces of the original central portions can be perceived. The whole substance displayed by the section is a dense clay ironstone, with a few traces of portions of bones.

It would be presumptuous in me, from such a fragment of an animal as the one now contemplated, and without the aid of figures of better preserved specimens, to pronounce with respect to the genus or even family of fishes to which it ought to be referred. That the original was an inhabitant of fresh-water, is rendered extremely probable, from the character of the stratum in which the relic was preserved. It was found in the bed marked No. 59. in the section of the main coal-pit of the county of Clackmannan, by my friend Mr Robert Bald, as published in the Memoirs of the Wernerian Society, vol. i. p. 498. The bed consists of clay-ironstone and cannel coal. It abounds with the remains of vegetables, and in some places contains numerous examples of bivalve shells, which have been denominated by the miners *tobacco leaves.* The shells are usually so compressed, that I have not been able distinctly to recognise the genus to which they should be referred, though I am inclined to consider them as connected with *Unio.* Detached scales, of the same form as those belonging to the example before us, have been observed in other beds of the same coal-field, accompanied by scales of a different character, by bones and by conical teeth, sulcated externally, and hollow internally at the base.

In all the beds, connected with the one which yielded the object before us, and these have been more or less exposed to the depth of 707 feet, there has not been a trace of organisms of marine origin detected; while vegetable remains, usually considered of land or fresh-water origin, are, as I have said, very generally distributed. In such circumstances, I have therefore little hesitation in considering the present organism as having belonged to a *lacustrine* fish, since all the strata of the coal-field at Clackmannan are of lacustrine origin.

As M. Agassiz is at present successfully occupied in the description and classification of fossil fishes, and may probably be enabled, satisfactorily, to establish a genus to which the present organism may be suitably referred, I feel inclined, provisionally, to denominate it ICHTHYOLITHUS CLACKMANNENSIS—a designation suggested by many pleasing recollections of kindness which I experienced while resident in the district of Clackmannan.

It was my intention to have sent you, along with this notice, sketches of several scales of fishes, of different kinds, together with the teeth, and also bones which I possess, and which I have procured from the mountain limestone, and the beds above and below that interesting formation. I regret, however, that I am compelled by particular circumstances to delay the communication for the present. Faithfully yours,

KING'S COLLEGE, ABERDEEN, JOHN FLEMING.
8th June 1835.

Remarks on Certain Statements of Mr Faraday, contained in the Fourth and Fifth Series of his "Experimental Researches in Electricity." By JOHN DAVY, M. D., F. R. S.* Communicated by the Author.

IN the Fourth Series of the Researches above named, published in the Philosophical Transactions for 1833, at page 511, Mr Faraday has made the following statement in a note.

"In 1801, Sir H. Davy knew that 'dry nitre, caustic potash and soda, are conductors of galvanism when rendered fluid, by a high degree of heat' (*Journals of the Royal Institution*, 1802, p. 53); but was not aware of the general law which I have been engaged in developing. It is remarkable that, eleven years after that, he should say, 'there are no fluids known except such as contain water, which are capable of being made the medium of connexion between the metal or metals of the voltaic apparatus."—*Elements of Chemical Philosophy*, p. 169.

In the Fifth Series of his Researches, published in the same volume of the Transactions, Mr Faraday quotes this passage a second time at page 681; and also another passage from my brother's Elements of Chemical Philosophy, that "when any substance rendered fluid by heat, consisting of water, oxygen, and inflammable or metallic matter, is exposed to the wires of the voltaic battery, similar phenomena of decomposition occur." And, in the next paragraph, Mr Faraday adds, "In 1826 he (Sir H. Davy) also pointed out that bodies not containing water, as fused litharge and chlorate of potassa, were sufficient to form, with platina and zinc, powerful electro-motive circles; but he is here speaking (continues Mr Faraday) of the production of electricity in the pile, and not of its effects when evolved; nor do his words at all imply that any correction of his former distinct statements relative to *decomposition* was required."

Now, as in these statements Mr Faraday does not appear to me to be just to my brother, nor correct, I trust the Royal Society will allow me to offer a few remarks in vindication, in doing which, I shall be as concise as possible, from the conviction, that the Philosophical Transactions ought to be devoted rather to objects of original research than to any thing bearing the character of controversial writing.

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* Read before the Royal Society of London, on 22d January 1835.

Relative to the first quotation, given from the Elements of Chemical Philosophy, respecting which Mr Faraday expresses surprise, comparing it with what my brother had observed eleven years before of the conducting power of fused nitre and caustic potash and soda, there appears to me to be no inconsistency, and consequently nothing remarkable. In the passage referred to, the subject under consideration is not the power of bodies to conduct voltaic electricity, but the nature of construction of voltaic batteries, such as were then known and in use,—the instrument, not the principle of action. If the passage be read in this sense, limited, as I conceive it was intended, there is, as I have before observed, nothing discordant with the facts relative to the known conducting power of the fused substances. And confirmation of this sense, it appears to me, is afforded in the second quotation from "The Elements," concerning the decomposition of bodies by electricity, such as consist of water, oxygen, and inflammable or metallic matter in fusion. Their decomposition in the manner noticed, implies conducting power, and of course a knowledge of that power being possessed by them when rendered liquid by heat. And, that my brother was fully possessed of this knowledge, is clear to demonstration from his writings generally, and especially from what he advances in his last Bakerian Lecture, that for 1826. I shall quote the whole passage, of which Mr Faraday has given only a part, especially as my brother in it expressly opposes a current opinion relative to the influence of water in voltaic combinations, which Mr Faraday himself has combated, but in a manner to give the idea that it was defended by my brother, and peculiarly his. The passage is the following:

"As most of the fluids which act powerfully in voltaic combinations contain water, or oxygen and hydrogen, it has been suspected that these principles were essential to the effect; this, however, does not seem to be the case, for I found zinc and platinum formed powerful electro-motive circles in fused litharge, and fused oxy-chlorate of potassa, which are not known to contain water; and (he continues) I have little doubt that similar effects would be produced by other fused salts containing only acid and alkaline matter," which Mr Faraday's extended trials have confirmed.

What can be more clear than this,—that my brother did not consider water as essential to the formation of a voltaic combination? What can be more clear that he was acquainted with the fact, that certain substances, non-conductors when solid, become conductors when rendered fluid by heat; and that he believed that the property was common to other salts?

But Mr Faraday states that my brother, in this passage, "is speaking of the *production* of electricity in the pile, and not of its effects when evolved; "nor (Mr Faraday adds) do his words at all imply that any correction of his former distinct statements relative to decomposition was required."

This criticism does not appear to me just. Mr Faraday is well aware that my brother considered chemical action and decomposition as essential to the action of voltaic combinations. How could the fused litharge and chlorate of potassa have acted, unless they underwent decomposition? It might as well be supposed that they acted without conducting. Is, therefore, Mr Faraday justified in wishing it to be understood that my brother considered the fused saline bodies as not undergoing decomposition? Surely it is not proper to judge of an author by detached sentences; his general views ought to be taken into account; and particular passages should be read and interpreted, if required, according to those views, not the contrary, as Mr Faraday has done, sacrificing the general views to particular passages, nowise to the advantage of the author; which I am the more surprised at, as Mr Faraday cannot plead ignorance of these views, believing him thoroughly conversant with the writings in which they are contained; and knowing how intimately he was acquainted with them from their living source. Now, considering my brother's general doctrines on voltaic action, and on electro-chemical decomposition, I see no shadow of reason for the conclusions which Mr Faraday has arrived at, or the assertions he has made. The first proof that my brother had of decomposition of the fixed alkalies, was in operating on them in a fused state by voltaic electricity. He witnessed the separation of their metallic bases at one extremity of the battery; and detected the evolution of oxygen at the other. At that time it was not known that the caustic alkalies are hydrats; but as soon as the fact was established, he opposed an hypothesis partly founded on it,—that potassium and sodium are hydrurets, and that the decomposition of water is essential to their formation. His doctrine of the production of voltaic electricity was,—that it is the result of the contact of bodies in different electrical states, or becoming so on contact, acted on chemically by imperfect conducting agents interposed, to which water is not essential; and to this I would refer, declining entering further into particulars against Mr Faraday's statements.

In defending my brother, I would not wish to detract from the real merits of Mr Faraday. In the papers in question, he has brought forward much original information, and has made us better acquainted with the powers of bodies in relation to the conducting of electricity. And yet, even in this part of his subject, he cannot be considered as just to my brother. My brother had not only ascertained that certain bodies, as nitre, the fixed alkalies, litharge, chlorate of potassa, from being non-conductors in their solid state, became conductors on fusion, but that the same property is possessed by earthy compounds and oxides generally.* He had also found and pointed out many bodies which are non-conductors when liquid, as sulphur, phosphorus, the combinations of these bodies and chlorine, the perchloride of tin, the chlorides of antimony and arsenic, the sulphuret of carbon, the chloride of azote,† and boracic acid.

In Mr Faraday's augmented list of fluids which are non-conductors, some of those bodies are mentioned as having been tried by him, without reference to the labours of any former inquirers; and, even on the fact of the loss of conducting power on the conversion of water into ice, with which he opens his dissertation (the fourth series), a fact on which the inquiry that followed was founded, he speaks of it as a discovery of his own; he makes no mention of any prior experiments on the subject; not even of Franklin's.* Mr Faraday, in a note already given, says Sir H. Davy was not aware of the general law he was then engaged in developing; that is, to use his own words, "the assumption of conducting power by bodies as soon as they pass from the solid to the liquid state." Were this general, it might be admitted as a law; but, as it is not, how is it a law? My brother, I have shewn (to confine myself to him), was acquainted with the fact that certain bodies, and these not a few, acquired conducting power on fusion; but, other bodies not doing so, it would have been illogical to have deduced such a law. Mr Faraday's *new facts* are valuable; the law he has deduced from them does not seem deserving of the name: it involves no general principle,—it insures no general results,—it throws no new light on the conducting power. The most perfect conductors are metals (whether solid or liquid, the form not being apparently concerned); diamond, and charcoal that has been well ignited, how opposite are they in conducting power!—how opposite the latter and common charcoal!—but why, we know not. Till these and other facts of a like kind are explained, attempts at the formation of general laws on the subject can hardly fail of being vain.

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* "The alkalies and the earthy compounds and the oxides, as dry as we can obtain them, though non-conductors when solid, are, on the contrary, all conductors when rendered fluid by heat."—*Phil. Trans.* 1809, p. 101.

† "The compound of chlorine and azote (he says) agrees with the compounds of the same substance, with sulphur, phosphorus, and the metals, in being a non-conductor of electricity."—*Phil. Trans.* 1813, p. 250. And, in an earlier paper, he says, the liquid compounds of sulphur and phosphorus and chlorine "are non-conductors in the same degree as oils."—*Phil. Trans.* 1809, p. 101. In the same page, of boracic acid, he says, "it is a good conductor as long as it contains water; but freed from water, and made fluid by heat, it is then a non-conductor."

I would willingly stop here, but I feel it incumbent on me to proceed a little farther, to Mr Faraday's notice of my brother's hypothesis respecting electro-chemical action, to which I think he is also deficient in doing justice, and in stating it correctly. In his fifth dissertation, at page 684 of the Transactions, Mr Faraday, speaking of electro-chemical effects described in Sir H. Davy's first Bakerian Lecture, says, "The mode of action by which the effects take place is stated very generally, so generally indeed, that probably a dozen precise schemes of electro-chemical action might be drawn up, differing essentially from each other, yet all agreeing with the statement there given."

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* Franklin, in his suppositions and conjectures towards framing an hypothesis for the explanation of the aurora borealis, has preceded Mr Faraday in some of his conclusions. He says: "15. A certain quantity of heat will make some bodies good conductors that will not otherwise conduct.

"16. Thus wax rendered fluid, and glass softened, by heat, will both of them conduct.

"17. And water, though naturally a good conductor, will not conduct well when frozen into ice by a common degree of cold; not at all when the cold is extreme."—*Franklin's Works*, 8vo, Lond. 1806, vol. ii. p. 71.

And Bergman appears ultimately to have satisfied himself of the correctness of Franklin's statement respecting ice, though he doubted it at first: he was of opinion, says Priestley, "if he could procure plates of ice of a proper thickness, he could charge them in the same manner as glass."—*Hist. of Elect.* part. i. p. 206. 4to edit.

Mr Faraday continues:—"When Sir Humphrey Davy uses more particular expressions, he seems to refer the decomposing effects to the attractions of the poles. This is the case in the 'general expression of facts' given at pp. 28 and 29 of the Philosophical Transactions for 1807: also at p. 30. Again, at p. 160 of the Elements of Chemical Philosophy, he speaks of the great attracting powers of the surfaces of the poles. He mentions the probability of a succession of decompositions and recompositions throughout the fluid, agreeing in that respect with Grotthuss; and supposes that the attractive and repellent agencies may be communicated from the metallic surfaces throughout the whole of the menstruum, being communicated from one particle to another particle of the same kind, and diminishing in strength from the place of the poles to the middle point, which is necessarily neutral."

Of the vagueness of my brother's views of electro-chemical decomposition, admitting, as Mr Faraday thinks, of a dozen precise schemes being drawn up from them, this is the first time I have heard mention. During his lifetime, they do not appear to have been so considered; no systematic writer that I am aware of, found with them fault of this kind, or was at a loss to ascertain their meaning precisely. Mr Faraday indeed does not call them *vague*, but *general*. The context, however, clearly proves this is his meaning: for a statement may be both of the greatest generality and of the greatest precision, but then it can have only one meaning, and cannot admit of a dozen different interpretations.

My brother, in his last Bakerian Lecture, remarks, that the same theory of electro-chemical action had been the guide and foundation of all his researches for twenty years; that is, from the time he first advanced it in his Bakerian Lecture for 1806. Considering it in this point of view, it would have been extraordinary indeed, were it as represented by Mr Faraday. As I have understood the fundamental theory, and as I believe it has been commonly understood, the vagueness of generality is not attributable to it. He (Sir H. Davy) considered all bodies as possessed of electricity, or capable of exhibiting electrical effects, some of positive, others of negative electricity in relation to each other,—that those bodies, similarly electrical, repelled each other,—that those dissimilarly electrical attracted each other,—that this latter attraction might be identical with chemical attraction ; the former (the electrical), acting on masses; the latter (the chemical) acting on particles ; and, that chemical combination might be promoted, and chemical decomposition might be effected, electro-chemically ;—in one instance by heightening the opposite electrical states of bodies brought together for chemical union ;—in the other, by opposing the opposite electrical states of the elements combined, and overcoming them by the influence of electricity artificially produced.

This theory, of which the above is an imperfect outline, if I do not deceive myself, is not less precise than the one which Mr Faraday has advanced. Indeed, his appears to be little more than a modification of it, in which he has transferred the centre of action from the boundary electrified surfaces, or the terminal points of the electrical apparatus conveying the electricity from its source, to the particles acted on ; with, however, this difference, as it appears to me at present, that the former is less obscure, and not less accordant with the greater number of facts.

My brother, it should be remembered, offered his views on the subject hypothetically. This he did in his first Bakerian Lecture, and in his last ; he attached to them no undue importance,—" believing that our philosophical systems are very imperfect, (I give his own words), and confident that they must change more or less with the advancement of knowledge." I do not defend them, therefore, on the idea that they are absolutely true,—merely on the ground that they have not been justly represented, and that their author has not had justice done him : and this defence has appeared to me the more necessary, as Mr Faraday, in the natural course of feeling by the world, might be considered his advocate, even keeping within the just limits of the excellent motto of the Royal Society, *Nullius in verba*, without viciously extending it to *facta*.

Even where Mr Faraday endeavours to be just, at least in one instance, he is hardly so. In a note, in his fifth dissertation, at page 680, he refers to an experiment of my brother's, given in his Bakerian Lecture for 1806, in confirmation of Dr Wollaston's experiments in proof of the identity of the electricity of the common machine and of the voltaic battery. After noticing it, and how oxygen and hydrogen were obtained *separately* from each other; and saying, had he known of it, it ought to have been quoted in an earlier series of his researches, he adds, " but it does not remove any of the objections I have made to the use of Wollaston's apparatus as a test of true electrical action." This conveys the idea that the experiment was not decisive, and that Dr Ritchie's objections to the original experiment of Dr Wollaston (that the electricity, like a dart, might cleave a molecule of water, and so disjoint mechanically its elements *), was applicable to it; which is not the case, as the gases were separately obtained,—hydrogen at the point connected with the positive conductor. This experiment is given by my brother in a *note*, as mentioned by Mr Faraday; but Mr Faraday takes no notice of the text, in which other results in proof of the identity of the electricities are described, as the evolution of potash round the negatively electrified point (operating on a solution of sulphate of potash), and sulphuric acid round the positive point; and, moreover, the transfer of sulphuric acid through moist asbestus into water; " so that (to use my brother's words), there can be no doubt that the principle of action is the same in common and the voltaic electricity †,"—a conclusion Mr Faraday has drawn from his own researches alone, as if those of my brother had never been made, and not acknowledging them even when he was aware they had been made.

I have the less hesitation in offering these remarks to the Royal Society, confident that, like the Academicians del Cimento, our best and earliest examples in scientific research, they have always been anxious, " Storicamente narrare, e di non defraudar mai gl'inventori di esse (esperienze) dell' invenzione, e della lode."* Persuaded as these academicians appear to have been, it was by only so doing that they could keep to another excellent rule which they had set themselves, viz. to experiment and relate, and avoid all disputation.

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* Phil. Trans. 1832, p. 282.

† Ibid. 1807, p. 31.

MALTA, *November* 4. 1834.

On the Temperature of some Fishes of the genus Thynnus. By JOHN DAVY, M. D., F. R. S., Assistant-Inspector of Army Hospitals.† Communicated by the Author.

IT is commonly believed and asserted by naturalists, that fishes generally, and without exception, are cold-blooded; thus Linnæus, in his "Regnum Animale," characterizes them in relation to their blood, by "sanguine frigido;"‡ and Baron Cuvier, our latest and highest authority, not only admits, but undertakes to shew, that it must be so; thus in the chapter of his "Histoire Naturelle des Poissons," on the general character and essential nature of fishes, he says, "Ne respirant que par l'intermède de l'eau, c'est-à-dire, ne profitant pour rendre à leur sang les qualités artérielles, que de la petite quantité d'oxigene contenu dans l'air melé á l'eau, leur sang a dû rester froid."§

It was many years ago, on a voyage to Ceylon, that I first met with an exception to this universally received opinion: it was in the instance of the Bonito (Thynnus palamys, Cuv. and Valen.), whose temperature was 99° of Fahr. in the deep-seated muscles in the thickest part of the fish a little below the gills, when the surface of the sea, from which it had just before been taken, was 80.5, the difference being the remarkable one of eighteen degrees and a half.

This fact necessarily made a strong impression on my mind, and a year or two ago, when examining the heart and gills of the tunny of the Mediterranean (Thynnus vulgaris, Cuv. and Valen.) my attention was recalled to it, on finding that the latter were supplied with nerves of unusual magnitude; that the heart like that of the bonito, was very powerful; that the fish equally, or

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[*] Saggi di Naturali esperienze fatte nell' Academia del Cimento, p. xxx.

[†] Read before the Royal Society of London.

[‡] Systema Naturæ, tom. i. p. 18.

[§] Hist. Nat. des Poissons, tome i. p. 275.

even more, abounded in blood, and that its muscles, generally like those of the bonito, from the same cause, were of a dark red colour. It immediately occurred to me, that its temperature also might be high, and the result of careful inquiry amongst the fishermen of most experience in the tunny fishery confirmed the conjecture. All who were asked, declared that the tunny is warm-blooded, and one of the most intelligent of them, when questioned as to the degree of heat, said it was much the same, or little less than that of the blood of a pig, when flowing from the divided vessels of the neck in being killed. And this man was very competent to give an opinion on the subject, having been much employed in the fisheries on the Sicilian coast, in which the viscera of the fish are the perquisite of the common fishermen, and are immediately taken out when the fish are caught.

From the tunny, I extended my inquiries to other fish of the same family, and learnt, that the analogy holds good, applied to all the species of the genus Thynnus of Cuvier and Valenciennes, with which the Maltese fishermen are acquainted, viz., (besides the two already mentioned) T. brevipinnis, T. thunnina, and T. alalonga, all of which abound in blood, have a powerful heart, red or reddish muscles; and also, as I have ascertained by particular examination, have their gills amply provided with large nerves. Not having been able to procure any of these fish alive, their exact temperature, of course, I have not been able to determine; but from the reports of the fishermen, it would appear that the common tunny is the warmest of the species; and in accordance with this, I have found its branchial nerves proportionally largest.

These nerves (the branchial), immediately after quitting the cranium, enter or swell out into ganglia of considerable size, and more or less connected together, from which five principal trunks proceed, the first four chiefly to the branchiæ, and are the respiratory nerves; the fifth, the lowest, to the branchiæ and stomach. In point of magnitude, those respiratory nerves almost rival the electrical nerves of the torpedo, and their origin is very similar, and their direction and associations, but with this marked difference between them, that the torpedinal nerves are entirely destitute of ganglia.

The respiratory nerves of the other species of thynnus which I have examined, very much resemble the preceding. Those which are smallest belong to Thynnus brevipinnis, and yet, even in this fish, in comparison with the fish of other tribes, the respiratory nerves are large, and their ganglia considerable. This fish, perhaps, may be considered as a link between the tunny family and the mackerel on one side, and the pelamides on the other; and the respiratory nerves of one of each of those genera of fish, which I have dissected, viz., of Scomber pneumatophorus and Pelamys sarda, have approached in magnitude those of the thynnus last mentioned. What the temperature of those fish is, I have not had an opportunity of determining by trial; according to the statement of the fishermen I have consulted, they are all cold-blooded. Reasoning from analogy, the natural inference is, that they will be found to be of somewhat higher temperature than other fishes less amply supplied with respiratory nerves.

As regards the rationale of the high temperature of the thynni, there appears to me less difficulty, than in accounting for the electrical power exercised by the torpedo, and other electrical fish. The peculiar function of the latter is performed by means of a particular organ, the most striking feature of which is a vast apparatus of nerves; but this organization bears little or no analogy to any other natural, or to any artificial process hitherto known, by which electricity is generated. Not so the respiratory apparatus, and associated organs in these fish of high temperature; they are essentially analogous in organization to the warm-blooded animals of the other two classes, and hardly more different from those of the mammalia, than those of the mammalia are from the respiratory apparatus and associated organs of birds. The function of respiration in water is commonly considered the same as in the atmosphere; the same change, it is supposed, takes place in the blood; the same change is ascertained to take place in the air dissolved; and, increase of temperature in one instance and the other, is referred to the conversion of carbon into carbonic acid. The difficulty is not as regards the kind of effect, but the degree of that effect; not an augmentation of one or two degrees above the temperature of the surrounding medium, but of many degrees. The considera_ tion of some of the peculiarities of these fish may help to diminish this difficulty, which I have little doubt will be removed entirely when we are better acquainted with their structure, and better acquainted with all the sources of animal heat.

The most important peculiarities are, I believe, chiefly the following:—A large and powerful heart; abundance of blood; large gills; and a very large apparatus of branchial nerves;—all which may be considered as concerned, either directly or indirectly, in the generation of heat: and the circumstances for its preservation are hardly less remarkable, as the manner in which the gills are defended by peculiarly strong opercula, abounding in fatty matter, and the deep situation of the principal bloodvessels, surrounded by thick muscles, and in addition, the aorta surrounded by the principal abdominal viscera, the kidneys, stomach, and liver. Moreover, the habits of these fish may, in some measure, contribute to their high temperature. They are frequently to be seen near the surface, and seem to have a delight in springing into the atmosphere. Aristotle, speaking of the tunny, says, of all fish it most enjoys warmth, and on that account swims near the surface, and frequents sandy shores. I quote from the old Latin translation of Theodore Gaza. "Thunni omnium maxime piscium gaudent tepore et ob eam rem arenam et littora adeunt; per summa etiam maris innatant, quo teporis potiantur;" *—confounding, as I believe in this instance he has done, a fondness for warmth, with the habits connected with its production. In this enumeration of circumstances which may contribute to the high temperature of these fish, both as regards its generation and preservation, I have intentionally been very general. In the present state of our knowledge, I apprehend it would be useless to be more minute. It is not improbable that these fish may possess means for generating heat peculiar to themselves, and of which at present we have no adequate idea; and the situation of the kidneys, of which a considerable portion is even higher than the stomach, and posterior to the gills, of large size, and abounding in blood, and well supplied with nerves, would lead to the conjecture that these organs, in the function of imparting heat, may possibly act a part.

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* De Hist. Animal, lib. viii. cap. 19.

Still, however, reflecting on the great proportional size of the branchial nerves, and guided by analogy, it is difficult to resist the conclusion, that they are not principally concerned in the performance of the function in question, and that these nerves, as means, are so very ample on account of the element inhabited, and the proportionally greater energy of function required to produce the same effect of elevation of temperature in water and in the atmosphere. On any other view, it seems difficult to account for the branchial nerves of these fish being proportionally very much larger than the pulmonary nerves of the mammalia, and vastly larger than those of birds, of all animals the warmest.*

Whether there is any immediate relation between the ganglia on the branchial nerves and the generation of heat in these fishes, is uncertain, and must necessarily remain so, as long as there is any doubt concerning the use of ganglia. The absence of ganglia on the principal nerves of the lungs of man, † and I believe of the mammalia generally, and of many birds, would lead to the inference that the nerves, in the instance under consideration, rather than the ganglia, are chiefly instrumental; and that the latter are in some way subservient to the former; but whether for giving sensation to the branchiæ, or for imparting extraordinary secreting power, so as to change the blood, or for some other purpose, remains to be ascertained.

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* The size of the pulmonary nerves of birds, and indeed of their respiratory nerves generally, so far as my observations have extended, is so small as to be truly astonishing, compared with their very high temperature. And, on the hypothesis of nervous influence being essential to the production of animal heat, through the agency of respiration, the necessary inference seems to be, that birds require less of this influence than any other description of warm-blooded animals, owing to their peculiarities of structure, both in relation to the diffused aërial means they possess of generating heat, and their peculiar means, in their covering of feathers, of preserving it; and, owing probably farther to their less expenditure of it, from the peculiarities of some of their principal secretions, especially those of the kidneys, skin, and lungs; their kidneys secreting an almost solid urine; their skin exhaling little moisture, and that not in sweat, but entirely by insensible perspiration; and their lungs, though exhaling more, from the nature of the function they perform, yet less than might at first be supposed, part of the aqueous vapour contained in the air expired being, I believe, condensed before it enters the atmosphere by the trachea, mouth, and beak, especially the latter, which are always comparatively cool. And, in accordance with this view, from the experiments of Messrs Allen and Pepys, it appears even, in one instance at least, that one of the warmest of birds, the pigeon, consumes, in relation to its bulk, less oxygen, or produces less carbonic acid, than a quadruped, the Guinea pig, the temperature of which is several degrees lower.

† Haller, speaking of the great sympathetic nerve, which in man is so amply provided with ganglia, says, " In pectore notabiles ramos paucos edit. Neque memini me alicujus momenti truncos vidisse qui ad nervum octavi paris accederent; etsi ejusmodi nervi illustribus viris visi sunt."—*Element.*

In concluding, I would remark, I am very sensible that this paper is in many respects greatly imperfect. I have been induced to contribute it chiefly with the view of calling attention to the subject, with the hope that others will engage in the inquiry as opportunities may offer; and, by more extended and minute observations, supply many desiderata, as the exact temperature of the different species of thynnus, the temperature of the blood coming from and returning to the gills, the minute peculiarities of the structure of these fish, and their habits. It seems not improbable that the investigation, if followed up, may not only throw light on the function of respiration in these fish, and on the production of their high degree of temperature, but also that it may aid in elucidating some obscure parts of the theory of respiration in general, in connexion with that of animal heat, especially the question, Whether any oxygen is absorbed by the blood in the lungs, and carried into the circulation.

Physiol. iv. 260. And, according to Sir Charles Bell's views of the nervous system, none of the respiratory nerves are ganglionic nerves. Sir Everard Home (*Phil. Trans.* 1825, p. 257), has endeavoured to associate the production of animal heat directly with ganglia; but the instances he has adduced seem liable to great objection; and the fact that the great sympathetic nerve in birds, is comparatively little developed, even less than in some reptiles, and destitute of large central ganglia, such as the semilunar in the mammalia (at least in every instance in which I have carefully sought for them, I have been unsuccessful), seems fatal to his hypothesis.

MALTA, *January* 8. 1835.

Work of Agassiz *on Fossil Fishes.*[*]

Of the great work on "Fossil Fishes," by Professor Agassiz, four numbers have already appeared, eminently distinguished by the accuracy and elegance of the engravings, and the very interesting nature of the letterpress. The fifth number is finished, and will appear during the course of next month.

In the first number our author informs us, that, by an attentive examination of the *scales*, fishes may be divided into orders more natural than those hitherto adopted by naturalists. In this manner he has established four orders, which bear some relation to the divisions of Artedi and Cuvier, but one of which, hitherto misunderstood, is almost exclusively composed of genera whose species are found only in the older formations of the crust of the earth. These four orders are the following:—

Order I. Placoides.—The tribes of this order are so named on account of the irregularity of the solid parts of their integuments; these are, masses of enamel, often of considerable size, or sometimes reduced to small points, as in the shields of the ray and the different kinds of shagreen of the shark. It comprehends the cartilaginous fish of Cuvier, with exception of the sturgeon tribe.

Order II. Ganoides.—This order comprehends families apparently very different from each other, but which, notwithstanding, when minutely examined, have many points of agreement. The character common to them all is the angular form of their scales, which are composed of two substances, one of corneous or bony laminæ, superimposed on each other, and covered with a thick coat of enamel. These scales are constructed precisely like the teeth. In this order are arranged the *Lepidoides*, Agass., all of which are fossil; the *Sauroides*, Agass., fossil, with the exception of two genera, viz. the Lepidostees and the Bichir; the *Pycnodontes*, Agass., also fossil; the *Sclerodermes, Gymnodontes, Lophobranches, Goniodontes, Siluridæ*, and *Sturiones*.

Order III. Ctenoides.—In this order the common character consists in their laminated scales being toothed at their posterior edges —those which are externally visible. The teeth of these numerous laminæ, which are so placed above each other that the lower always project over the upper, make the scales rough to the touch. This structure is particularly obvious in the *Chetodontes* and *Pleuronectes*. In this order are arranged the *Percoides, Polyacanthes, Scienoides,*

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[*] Recherches sur les Poissons Fossiles, in quarto livraisons; the plates in folio. The booksellers in London who furnish the work are Black and Armstrong, 2. Tavistock Street, Covent Garden, and J. B. Bailliere, 219. Regent Street, London.

Sparoides, Scorpenoides, and *Aulostomes.* There are the Anthopterigians of Cuvier and Artedi, with exception, however, of those having smooth scales, and with the addition of the Pleuronectes.

Order IV. Cycloides.—The families belonging to this order are provided with scales formed of simple laminæ with smooth edges, a circumstance which does not prevent their external surface being frequently ornamented with different designs, which are imprinted on all the laminæ, where they are exposed to view and are not covered over. The scales of the lateral line are formed like all the others; but, in place of being mere laminated plates, these are funnels, placed the one within the other, and of which the narrow part, applied to the disc of the scale, forms a tube, from which the mucus which covers the fish is poured out. This tube is sometimes bifurcated or even ramified. In this order are placed the *Labroides, Muges, Atherines, Scomberoides, Gadoides, Gobioides, Murenoides, Lucioides, Salmones, Clupes,* and *Cyprinidæ.*

If we estimate the number of species of fishes, now known to amount to about 8000, we may state that more than three-fourths of this number belong to two only of the above-mentioned orders, namely *Cycloides* and *Ctenoides,* whose presence has not been discovered in the rock formations below chalk. The other fourth part of living species is referable to the orders *Placoides* and *Ganoides,* which are now far from numerous, but which existed during the whole period which elapsed since the earth began to be inhabited, to the time when the animals of the greensand lived. The remarkable conclusion to which M. Agassiz had come from the study of more than 600 fossil fishes on the Continent, has been corroborated by the inspection of more than 250 new species, found in the British collections.

In the first, second, and third numbers there are descriptions and admirable figures of genera and species of the different orders, both Continental and British. General discussions also occur; one in particular we recommend to the attention of our readers, viz. that on the colours and scales of fishes.

The fourth number contains a critical review of the numerous tribes of fossil fishes found in the famous Monte Bolca, and a tabular view of the fishes of the chalk formation. M. Agassiz, in the same number, informs us that he obtained vast additions to his former stores in the British collections, of which the following are noticed: viz. British Museum, Museum of the Geological Society of London, of the College of Surgeons, and of the United Service Club; the beautiful collection of Mrs Murcheson; the cabinets of Messrs Lyell, Stokes, Fitton, Sharpe, Yarrell, and Richardson; all in the vicinity of London. Great additions were obtained from the collection of Dr Buckland and the splendid cabinets of Sir Philip Egerton and Lord Cole. The fine collection of Mr Witham, and the Museums of Whitby, Scarborough, York, Leeds, Birmingham, Liverpool, Bristol, also proved productive sources of new and interesting species. The private collections of Miss Philpot and Mr Cumberland, and the well known museum of Dr Mantell, contributed an ample supply of species entirely new to M. Agassiz. At Edinburgh the collections of the Royal Society and the College Museum; the cabinets of Professor Jameson, Lord Greenock, Dr Hibbert, Dr Traill, Mr Copland, and Mr Jameson Torrie, proved not less interesting than those visited by our author in England and Ireland. After enumerating the above and other collections in very courteous terms, he adds the following observations.

These notices concerning the rich and splendid materials with which, from so many quarters, I have been favoured during the past year, naturally suggest some additional remarks concerning that portion of our work which has been already executed, and also regarding that which still remains for the furtherance of the science of fossil fishes.

The study of Ichthyology has, in all past ages, been much neglected, in comparison with that of the other branches of natural history. The extreme difficulty which exists in observing fishes in their watery haunts, and in collecting authentic facts regarding their habits, and the whole of their animal economy, has rendered this science much less attractive than the history of the great mammiferæ, and of the feathered tribes. Even reptiles, hideous and ofttimes dangerous as they are, have found more admirers than fishes; and concerning the attractions of entomology and conchology, we need say nothing. In the midst of so many favourite fields of research, fishes have remained hid from us in the vast oceans which they inhabit, for the number of those already described is comparatively small; and if the great work on Ichthyology of Cuvier and Valenciennes promised us the description of from six to eight thousand species, the greater is our regret that the volumes which have hitherto appeared contain no more than a fifth part of the number. And now, notwithstanding all these attending difficulties, the first steps being taken, and an entrance effected into these new regions, what a world of wonders presents itself in the depths of the ocean, and in the inaccessible haunts of the creatures which inhabit it? In approximating to these results, we unfortunately cannot repose *confidence* on any guides whom we now possess, since the older amongst them reveal but a few species, and the best of the more modern, leave us in the midst of the investigation. Accordingly, I have had to pursue my researches, in a great degree, independently of every thing which was previously accomplished, in establishing an equilibrium in the various branches of Ichthyology, and in making the whole of this labour nothing more than a simple introduction to the examination of those fosil species I sought to determine; for it will now be readily conceived, that those memoirs concerning Ichthyolites which were published some twenty years ago, do not now exhibit results in keeping with the knowledge which may be easily acquired regarding the existing species, in the many great collections throughout Europe.

From this state of things, and from the manner in which I have been obliged to study living fishes, that I might compare them with fossil ones, a great advantage has resulted in the complete independence I was required to maintain concerning all the former reputed alliances of different fishes; because the great number of new species which have been discovered since the commencement of the present century, for the most part represented in the *Regne Animal* of Cuvier, and which it was necessary to insert in the groups of the natural families of this class, has caused all the alliances proposed by the older ichthyologists entirely to disappear. In afresh reviewing their characters, I have been led to adopt a classification which differs considerably from any arrangement which has hitherto been proposed, and which is founded upon important considerations which have hitherto been neglected.

It admits of no doubt, that one of the distinctive characters of the class of fishes, consists in the skin being possessed of scales of a peculiar form and structure. This covering, which protects the animal externally, has, according to all the observations I have made up to the present moment, the most direct relation to its interior organization, and to the external circumstances in which the animal is placed. So that, in this point of view, the scales acquire a primary importance, and may be regarded as a superficial reflection of all that passes within and around the fish. Accordingly, upon attentively examining them, I have found that fishes may be arranged in an order much more natural than any hitherto proposed, by allowing ourselves to be regulated by the structure of the scales. Acting on this principle, I have established four orders, which present some resemblance to the great divisions of Artédi and Cuvier, but one of which, hitherto almost wholly unknown, is nearly exclusively formed of genera, the species of which are found solely in the older strata of the crust of the globe. These four divisions are—the *Placoïdes,* which includes all the cartilaginous fishes of Cuvier, with the exception of the Sturgeons; the *Ganoïdes,* which comprehends more than fifty extinct genera, and with which it is necessary to ally the Plectognathes, the Syngnathes, and the Acipenser; the *Ctenoïdes,* which are the Acanthopterygiens of Cuvier and Artédi, to the exclusion, however, of all those which have smooth scales, and including with them the Pleuronectes; and, lastly, the *Cycloïdes,* which are principally the Malacoptérygiens, but which likewise comprehend all the families which are excluded from the Acanthoptérygiens of Cuvier, and from which it is necessary to separate his Pleuronectes, that they may be carried back to the preceding order.

The better to comprehend the general results which I propose to present, it will be necessary to say a few words on the existing species.

We are now acquainted with about 8000 species of fishes. Of this number more than three-fourths belong to two orders of that class, the presence of which has not yet been discovered in formations older than the cretaceous one, viz to the Cycloïdes and to the Ctenoïdes; so that, truly, there is nothing analogous to them in the whole series of the secondary rocks, even to greensand; whilst the remaining fourth belongs to the orders Placoïdes and Ganoïdes, not at all numerous now, but which existed *alone*, during the whole period which elapsed from the time the earth began to be inhabited, till the moment in which the animals found in the greensand appeared. The same precise proportion existing in the orders of the class to which attention is more especially solicited in this work, is a fact which is truly remarkable; it is almost inconceivable, but still undoubted, since it is a mere matter of calculation; and moreover, we may remark, it is not only in general that this regular arrangement of the groups may be noticed; but in each order, and in each family even, the genera produce in their affinities analogous series, so that the differences of organization become the distinctive characters for the geological epochs, even in those species which are seen for the first time. I can now state this result with confidence, after having reviewed the general conclusions to which I have arrived in the study of fossils, and supported by the examination of 250 new species discovered in British Collections, without having met with a single exception in the 800 species with which I am now acquainted. These essential organic differences have an especial reference to the nature of the integuments, and to the mode in which the vertebral column terminates in the caudal fin, in other words, to the relations which subsist in the animal to the material world which surrounds him and the structure of that organ which is essential to his locomotion. I shall now very briefly point out those distinctions, and, at a future time, will fully enumerate all the fishes belonging to each great formation; for in thus presenting a general description, it will easily be understood that I cannot enter largely into detail.

That we may appreciate at its just value the study of fishes in general, and of fossil fishes in particular, we ought never to lose sight of the true position of this class in the scale of living beings. Placed higher than the radiata and the mollusca, they present peculiarities of organization more numerous, and also subject to greater differences; and in them also we remark, within narrower geological limits, more marked differences than among animals lower in the scale. In the class of fishes, we do not see genera, nor even families, run throughout a whole series of formations with species which often differ but very little in appearance, as happens in the Zoophytes; on the contrary, this class, from one formation to another, is successively represented by very distinct genera, referable to families which themselves soon vanish, as if the complicated apparatus of a superior organization could not be long perpetuated without intimate modifications; or rather, as if animal life had a more rapid tendency to change in the higher orders of the animal kingdom, than in those lower in the scale. In this respect, it is with fish nearly as it is with the mammiferæ and reptiles, whose species, in general very limited, belong in the series of formations with little vertical distance, to genera which are different, without passing insensibly from one formation to another, as is often witnessed in certain shells. This is one of the most interesting facts which I have observed, namely, that I do not know a single species of fossil fish which is successively found in two formations, whilst I know a great number which have a considerable horizontal distribution. Besides, the class of fishes moreover presents to zoological geology the immense advantage of extending across all the formations, and of presenting in a class of vertebral animals a point of comparison regarding the differences which exhibit themselves in the longest lapse of known time, of animals constructed generally on the same plan—of animals of a class which already counts a very great number of fossil species,for the most part referable to types which exist no longer; and whose affinities with the living species are as distinctly marked as those which ally the Crinoidea to the ordinary Echinodermata, the Nautiles, and the Sepia to the Belemnites, and the Ammonites, the Ptérodactylæ, the Ichthyosauræ, and the Plesiosauræ to our Saurians; and the living Pachydermata to those which of old inhabited the borders of the lakes around Paris, or the plains of Siberia.

The fishes of the tertiary rocks, are those on which I the least dilate, because they approach nearest to the living species, and because their study may be undertaken by means of works which are already published upon Ichthyology. At the same time, considering the prodigious number of living species to which they approximate, it is often very difficult, in the condition in which they are discovered, to identify them, or rather exactly to appreciate their distinctive characters. I will only remark, in general, that up to the present moment, I have not found a single species which is perfectly identical with those of our seas, except that little fish which is found in Greenland, in the géodian clay, and the geological age of which is unknown to me.

The species of the Crag of Norfolk, of the superior subapennine formation, and of the molasse formation, for the most part approach to the common genera of tropical seas; such as the Platax, the Great Carcharias, the Great Myliobates, &c.

In the inferior tertiary formations,—in the London clay, the coarse limestone of Paris, and of Monte-Bolca, we find that at least a third of the species already belong to *genera* which no longer exist. In the comparative tables of all known fishes which I propose soon to publish, I shall give the names of all the fossil genera and species of all geological epochs, and at the same time shall point out all the localities in which they are found, and the corresponding genera of the actual existing creation, in a separate column.

The Chalk group includes more than two-thirds of those species which are referable to *genera* which have entirely disappeared; and here we already see some of those singular forms which prevail in the oolite series. At the same time, as a whole, the fishes of the chalk formation much more resemble the general character of the fishes of the tertiary series than that of those belonging to the oolite group; and this to such an extent, that did I regard only the fishes, in a general classification of geological formations, it would appear to me more natural to associate the chalk and greensand series with the tertiary formations, than to rank them with the secondary rocks. Underneath the chalk there is no longer a single genus which possesses existing species; and even those of the chalk which have them, possess a much greater number of fossil ones.

The oolite group to the lias, also included, forms a very natural and very well defined series, which must also comprehend the Wealden rocks, in which I have not found a single species referable even to the chalk genera. From that epoch, always descending, the two orders which prevail in the present creation are no longer found, whilst those which are in small numbers in our days suddenly present themselves in great abundance. As to the Ganoides, the genera with the symmetrical caudal fin are those which are here found; and among the Placoides, it is especially those with teeth furrowed on their two faces, and with large spinous rays, which predominate. For it is now certain that those great rays which Dr Buckland and M. de la Beche have denominated Ichthyodorulithes belong neither to the Silures nor to the Balistes, but are the rays of the dorsal fin of the great Squali, the teeth of which are found in the same beds.

In quitting the Lias, and proceeding to the lower formations, a great difference is observed in the form of the posterior extremity of the body of the Ganoides. All of them have the vertebral column at its extremity prolonged into an imperfect lobe, which reaches to the extremity of the caudal fin, and this peculiarity extends to the most ancient fishes. Another observation, which is worthy of remark, is, that down to the coal formation, no fishes that are evidently carnivorous are found; that is to say, with large conical and sharp teeth. The others appear to have been omnivorous, their teeth being rounded, or obtusely conical, or pencil-shaped.

The day is assuredly not far distant when we shall be able to collect a great number of facts respecting the habits of these animals, and their interior organization. The discovery of coprolites already enables us to discover the organized beings which preyed upon the pirates of the ocean; for in these coprolites, which are sufficiently numerous in those deposits which contain Sauroid fishes, we easily discover the scales of the fishes which they ate, and sometimes we can determine the scales. Their intestines even are preserved in some cases, as for example in a specimen of Megalichthys, where a portion of intestine is visible; and bundles of pyloric appendices, and the *cul-de-sacs* of intestines of the species Leptolepis and Thrissops of Solenhofen, known under the name of *Lumbricaria*, are not rare in the schists of this interesting locality. In the fishes of the chalk group, there may be seen, in Mr Mantell's collection, specimens of Macropoma, in which the whole stomach is preserved with its different membranes, which are separated, as it were, into as many leaves. In a great number of the fishes of Sheppey, of the chalk and oolite series, the capsule of the eye-ball is still preserved; and in many species of Monte-Bolca, of Solenhofen, and of Lias, there are very distinctly to be seen all the little laminæ which form the gills. It also appears evident that the constitution of some rocks is much more favourable for the preservation of animal remains than that of others.

It is in the series of deposits inferior to the Lias that we begin to discover the largest of those monstrous Sauroid fishes, whose osteology approximates them so much to the skeletons of the Saurians, by the stronger sutures of the cranial bones, by their great conical teeth with longitudinal striæ, and also by the mode in which the spinous apophyses are articulated with the bodies of the vertebræ, and the ribs also at the extremity of the transverse apophyses. The analogy between these fish and the Saurians goes further than the skeleton: in one of the two species which is now alive, I have found an interior organization of soft parts, which is very peculiar, and which approximates these groups of animals more than at first was anticipated. In the *Lepidosteus osseus* there is a real glottis, like that of the sirens and the salamander reptiles, also a cellular swimming bladder, with a trachea, as in the lungs of an ophidian. Finally, these integuments have often an appearance so like to those of the crocodile, that it is not always easy to distinguish them.

The small number of fishes found in the transition rocks appear not as yet to enable us to assign to them any peculiar character. At the same time, the species in the collection of Mr Murchison exhibit types which are not found even in the coal measures.

What is very remarkable in all the fishes below the oolite species, besides their resemblance to reptiles, is, on the one hand, the greatest uniformity of their types; and on the other, the very great uniformity of the parts of the same animal among themselves; so much so, that the scales, the bones, and the teeth of one are with difficulty distinguished from those of the other. And if we might here hazard a conjecture on this state of matters, we should be naturally led to fancy that the principle of animal life, which developes itself at a later period under the form of our common fishes, of reptiles, birds, and beasts, is, at first, entirely confined to these singular sauroid fishes, at the same moment participating of the nature of fishes and of reptiles; and that this mixed character is only lost in this class upon the appearance of a greater number of reptiles, as we see the Ichthyosaurus and Plesiosaurus participate, as to their bony structure, in the characters of the cetaceæ, of the class of mammiferæ; and the great terrestrial saurians to those of the Pachydermæ, which would appear to have been called into existence at a much later period.

We are thus by observation conducted to those ideas of the philosophy of nature which are presented to us by an organic and regular development of all created beings, in fixed relation to all the different conditions of existence which are realized on the surface of the globe, at the conclusion of those changes to which it has itself been subjected.

According to all these facts, I perceive in these series of all the geological formations two grand divisions, which have their limits at the greensand. The former, and more ancient, includes only the Ganoides and Placoides; the latter, more closely allied with the existing species, comprehends forms and organizations much more diversified; and these are principally the Ctenoides and the Cycloides, and a very small number of the species of the two preceding orders, which insensibly disappear, and the living resemblances of which are very considerably modified. As I do not find in the fishes of the first grand period the differences which correspond to those which we now observe between fresh-water and marine fishes, it appears we perhaps go farther than facts warrant, when we admit in the oolite series, and in those which are lower than it, distinct formations of fresh and salt water. I rather think that the waters of these remote periods, confined in less secure basins, did not then present the same striking differences which we observe in our own days.

Such is a very meagre sketch of a history of the deepest interest, and full of curious episodes, which are, however, somewhat difficult to relate. The exposition of the details which it includes will be the task of my life.

The fifth number contains chiefly histories and descriptions of genera and species met with in the formations of England and Scotland.* The plates in this number are even more admirably executed than those in the fourth livraison. The following enumeration of the new British species may prove acceptable to our geological readers:—1. *Palæoniscus.* Of this genus the following species are described: 1. *P. Robisoni*, found in the limestone of Burdiehouse, near Edinburgh, and by Professor Jameson in the Burntisland district. 2. *P. striolatus*, found at Burdiehouse and in Fifeshire. 3. *P. ornatissimus*, in the limestone of the Burntisland district. 4. *P. elegans*, in the magnesian limestone of Durham. 5. *P. comatus*, Durham. 6. *P. glaphyrus*, magnesian limestone. 7. *P. macrophthalmus*, magnesian limestone. 8. *P. longissimus*, magnesian limestone. 9. *P. carinatus*, in the clay ironstone of Wardie.

Lord Greenock found that many of the balls of clay-ironstone at Wardie, to the westward of Newhaven, contained, as a nucleus, coprolite, or portions of fish. He also found the following species of the genus Amblypterus at Wardie, viz. *A. ambnemopterus*, *A. punctatus*, *A. striatus*. Of the genus Osteolepis, the following species are mentioned as British: *O. macrolepidotus*, in the slate of Caithness and Pomena; 2. *O. microlepidotus*, in Caithness and Pomona; 3. *O. arenatus*, at Gamrie, in Banffshire. Of the genus Acanthodes, the *A. sulcatus* is mentioned as having been found by Lord Greenock at Wardie. Of the genus Cheiracanthus, the following species are enumerated as British: 1. *Ch. Murchesonii*, found at Gamrie by T. Jameson Torrie, Esq. and others; 2. *Ch. minor*, by Dr Traill in Pomona. Of the genus Cheirlepis, the following British species are described: 1. *Ch. Traillii,* found by Dr Traill in the slates of Pomona; 2. *Ch. Uragus,* found at Gamrie, where there is an interesting deposit of fossil fishes in a red sandstone, which, in the regular succession, is below the coal formation. Of the remarkable genus Cephalaspis, the following British species are mentioned, 1. *C. Lyellii,* of which fine specimens were found in the old red sandstone of Forfarshire many years ago by Professor Jameson, and since by Mr Lyell and others; 2. *C. Lewisii,* found at Whitebach; 3. *C. Lloydii,* common in the old red sandstone in Wales. The consideration of this genus leads our author to the following observations *on the strata in which the most ancient fish are found.*

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* Although the fifth livraison is not yet published, we have, through the kindness of the author, obtained a sight of it.

The truly astonishing character of the genus (Cephalaspis) afford me a renewed opportunity of remarking how much the several portions of the frame of the animals of the most ancient epochs, exhibit a uniformity of structure, and at the same time, types of the animal kingdom which are but little distinguishable among themselves. Here, for example, the bones of the head are all as one, the scales are united in very elevated bands, and the rays of the fins remain covered by the membrane which elsewhere surrounds them; whilst the whole animal, to a most extraordinary extent, resembles the Trilobites, which have somewhat preceded the *Cephalaspis* in the series of their creation. This example alone would suffice to manifest the constant laws which regulate the succession of living beings, and their progressive development, if the whole class of fishes were not itself a continual demonstration of it.

All the species of the genus *Cephalaspis* have been found in the old red sandstone of England and Scotland. It is not, therefore, as has been asserted, in the mountain-limestone, and consequently still less in the Zechstein or Magnesian limestone, that the vestiges of the most ancient fishes have been found. Their presence mounts up to an epoch yet more distant; for it is now certain that they are found in very considerable number in the old red-sandstone. And, moreover, even this formation is not the oldest in which fossil fishes have been discovered. But since the *Cephalaspis* itself belongs to an epoch so very remote, and since it is of the highest importance for the science of Paleontology to determine exactly the formation in which the first traces of fish are found, I deem it proper now to enter into some details concerning all the species of fossil fishes, the debris of which have been discovered in the most ancient beds of the crust of the globe, even although their organization will compel me, in part at least, to postpone their description to the 3d volume of this work.

In order that I may be able the better to fix and point out my indications concerning the strata in which the most ancient fish occur, and also that no uncertainty may remain concerning the geological age of their beds, it will be useful here to transcribe in a summary way the results of Mr Murchison's researches into the fossiliferous stratified rocks below the coal formation—and these indications will be the more exact, inasmuch as it is to Mr Murchison that I am indebted for the most of the specimens of fossil fish which I have examined, belonging to formations below the coal measures, and it is also his valuable communications which, in a great degree, have enabled me to digest this notice....In the synoptical table of these stratified deposits which Mr Murchison has published, he commences with the carboniferous limestone, and descends successively to the schistose system of the southern parts of Wales. The English old red-sandstone is the most recent formation whose beds are examined in detail in this table. The upper division of this formation is wholly destitute of organic remains.* The whole of these beds, formed of a red conglomerate, and of different sandstones, has a thickness of many thousand feet, as may be ascertained by visiting the escarpments—the slopes—of the counties of Brecknock and Caermarthen; they support the coal formation in the southern parts of Wales. The middle division of the old red-sandstone consists of red and green marls, with numerous beds of concretionary limestones called *cornstone*, and some beds of a very hard sandstone ; it is this part of the formation which contains the debris of the *Cephalaspis*. There has not been the slightest trace of any other kind of organic body discovered in this division of the formation, these fragments of fishes excepted, which, at the same time, characterise it in a very peculiar manner. They have been found by Mr Murchison himself in the different parts of this division, in the counties of Salop, Hereford, Worcester, Monmouth, and Brecknock, over an extent of nearly 3000 square miles, and always occupying the same geological horizon. This group of cornstones has likewise a very considerable thickness, equal probably to that of the division immediately above it. At the same time, Mr Murchison thinks that the remains of the fishes are especially abundant at the lower portion of this middle division. In the lowest division of the old red-sandstone, below the horizon of the *Cephalaspis*, Mr Murchison found, at Downton-Hall, near Ludlow, not more than the fragment of a head, with a portion of the scaly cuirass, evidently belonging to the *Dipterus Macrolepidotus ;* and at Tinmill, near Downton Castle, some small Ichthyodorulites, accompanied by a new species of *Pileopsis*, and a new species of *Avicula*. In this last locality, the transition from the old red-sandstone to the Ludlow-rock sandstone which it covers, was very distinctly perceived. The situations in which the fossils of this formation occur most frequently, are Whitbach near Ludlow, the Whyle, the Bromyard Road, Sutton-Hill, Downton-Hall, Menaibridge, and Abergavenny. Mr Murchison imagines that the concretionary nature of the limestones of the old red-sandstone formation, and their being broken into small pieces so as somewhat to resemble a conglomerate rock, has prevented the discovery hitherto of native fishes, but he does not despair of discovering them in the more compact sandstones. Neither has he found any in the vast masses of concretions which have somewhat of a subcrystalline structure, and an occasional thickness of about twenty feet. In Scotland, however, and, amongst other places, at Glammis in Forfarshire, some specimens of *Cephalaspis*, in fine preservation, and almost entire, have been discovered, and have been transmitted to me by Professor Jameson and Mr Lyell. Lastly, the scales of the old red-sandstone of Fifeshire, which have been described by Dr Fleming, belong to a gigantic species of *Gyrolepis.* Thus the old red-sandstone contains debris of many species of *Cephalaspis,* of a species of *Dipterus,* and of a species of *Gyrolepis,* (consequently of those genera which belong to the order Ganoides), and many species of *Ichthyodorulites,* which are the bony rays of different fishes of the order Placoides. These bony rays present such remarkable differences, that it is impossible to suppose that they have belonged to the same genus, any more than to any genus, the rays of which are found in the superior geological formations. I shall describe some under the denomination of *Ctenacanthus ornatus,* and others under that of *Onchus Murchisoni* and *Onchus erectus.*

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* Mr Murchison has lately discovered scales of fishes in the upper part of the old red sandstone.

In descending further into the greywacke group, the debris of fossil fishes are still to be found at different depths; and as the researches of Mr Murchison have led him anew to subdivide this series of deposits into many formations, I deem it expedient yet further to point out in this place the limits of his subdivisions, in order that we may accurately define the layer of the crust of the globe which incloses the first traces of the presence of fishes.

Immediately under the old red-sandstone we find the upper portion of the superior greywacke series, which Mr Murchison calls the *Ludlow rock,* and regards as the first formation of his series. This system is characterized in its upper subdivision, which Mr Murchison designates the Upper Ludlow rocks, by a new species of *Avicula,* and by the *Avicula retroflexa* of Hisinger, by a new species of *Atrypa,* a new species of *Cypricardia,* the *Homonolotus Knightii* (a new genus of Konig), the *Leptana lata* of Buch, by many new species of Orthis, two new species of *Orbicula,* different new species of *Orthocera,* two of *Pleurotomaria,* a new species of *Turbo* and *Gigantic serpulæ.* These fossils, which will be described and figured in the work now preparing by Mr Murchison, are contained in a slightly micaceous, grey-coloured, thin-bedded sandstone. The environs of Ludlow Castle, in Shropshire, and those of Croft Castle in Herefordshire, the western flanks of the Malvern and Abberly Hills in Worcestershire, the western slopes of May-Hill, Pain Castle, in Radnorshire, and the Treverne Hills, belong to this subdivision. In its central subdivision, the Ludlow-rock formation comprehends the Aymestry and Sedgley limestone, subcrystalline, grey and blue argillaceous limestone, characterized by the *Pentamerus Knightii,* Sow., the *Pileopsis vetusta,* Sow., a new species of *Bellerophon,* a species of *Lingula,* one of *Atrypa,* the *Terebratula Wilsoni,* Sow., the *Calamopora fibrosa,* Goldf., and some other corals. This subdivision is remarkably well developed near Aymestry in Herefordshire, in some parts of Shropshire, and at Sedgley in Staffordshire. The third and last subdivision is that of the lower Ludlow rocks; these are concretionary and sandy limestones, and sandy shales of a dark colour; and are especially developed in the escarpments of Monktree and Bridgwood in the valley of Woolhope in Herefordshire, in the escarpments in Montgomery and the Radnor Forest; and are characterized by three species of *Phragmoceras* (a new genus of Mr Broderip), by the *Asaphus caudatus,* Brogn.; two species of *Cardiola* (new genus of Brod.), a new species of *Nautilus,* two of *Spiralites,* a *Pentamerus,* the *Atrypa galeata,* Dalm., a new species of this same genus, one of *Pleurotomaria,* the *Orthocera pyriformis,* and many other fossils. The teeth of fishes have also been found in this formation, though not in great numbers, and lying most commonly in the upper Ludlow rocks. No part of the body of a fish was ever discovered in this formation till this present year, when the upper beds being removed at Ludford, in digging the foundation of some houses, a heap of scales were discovered, as also the rays of fins, and teeth completely broken, lying huddled together, and forming a bed between the strata of sandstone which contain an immense number of Serpulæ, also *Leptæna lata.* and of other fossils which characterize this formation.

These fragments are too incomplete, to enable us at the present moment to arrange them in a systematic table of classification. It is, however, certain, that they do not present any specific analogy to the fishes of the old red sandstone formation. The remains of the fins belong to a different species of Ichthyodorulites, and the scales appear to belong to different fishes of the family of Lepidoidians, for their appearance is very varied; the teeth, moreover, are less numerous, and none have been found entire. The nature of these strata, their disintegrated condition, and the fossils which they contain, have, altogether, led Mr Murchison to suppose that they have been deposited in very shallow waters. Only a very few Ichthyodorulites have been found in the lower Ludlow rocks; and since Mr Murchison has not collected them himself, he thinks that their presence in this subdivision should be admitted with some degree of hesitation; and the more so, as they have not been found in the Aymestry limestone, which contains such an immense quantity of organic debris, and amongst others of the *Pentamerus.*

The second formation, arising from the division of the greywacke group, is composed of the *Wenlock* and the *Dudley rock,* which swarm, so to speak, with corals, shells, and Trilobites.* Mr Murchison divides it into two subdivisions: the upper comprehends the Wenlock and Dudley limestone, which is a subcrystalline limestone, highly concretionary, and of a grey or blue colour. It contains an immense quantity of corals and crinoidea,—the *Bellerophon tenuifascia,* Sow., the *Euomphalus rugosus* and *discors,* Sow., the *Conularia quadrisulcata,* Sow., a new species of *Pentamerus,* also of the *Natica,* the *Natica spirata,* Sow., the *Leptæna euglypha,* Dalm., the *Spirifer lineatus,* Sow., and a new species of this last genus, the *Terebratula cuneata,* Dalm., the *Producta depressa,* Sow., many species of Orthocera, the *Asaphus caudatus,* Brogn., the *Calymene Blumenbachii,* Brogn., and other trilobites. This subdivision prevails remarkably in the neighbourhood of Wenlock, in Shropshire, in Caermarthenshire, at Dudley, and in Gloucestershire. The second subdivision comprehends the Wenlock and Dudley argillaceous shales, which are of a dark grey colour, rarely micaceous, and contain nodules of earthy limestone. In these strata are especially found a variety of the *Asaphus caudatus,* the *Calymene Blumenbachii,* a new species of *Lingula,* and a new species of *Orthis;* also the *Cyrtia trapezoidalis,* Dalm., a new species of *Delthyris,* an *Orthocera,* the *Orthocera annulata,* Sow., besides *Crinoidea,* &c. Mr Murchison has never found the least trace of fishes in this formation.

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* Mr Murchison now names this second formation the *Wenlock Limestone.*

Mr Murchison's third formation includes the *Horderley* and *May-hill rocks,* which descend as far as the Black Rocks of Llandeilo and Builth. It is entirely devoid of the debris of fishes, and is especially characterized by the *Pentamerus lævis*, Sow., the *P. oblongus*, Murch., a new species of *Leptæna*, one of *Pileopsis*, a new terebratula, many crinoidea, some corals, undescribed species of trilobites, including the genus *Cryptolithus,* which has been discovered in North America, and fourteen species of the genus *Orthis,* different from those which have been found in the overlying formations, including the *Orthis Callacis* of Dalm., and his *O. Aperturatus.* The *Asaphus Buchii,* Brogn., his genus *Agnostus*, and other undescribed species, characterize the Llandeilo and Builth rocks, which form the fourth formation of the series. *

By descending still lower, we arrive at the system of slaty rocks of the southern part of Wales, which Professor Sedgwick has examined with such minute care, and in which he has never discovered the least trace of fishes. Thus we have under the old red sandstone a *geological scale* of many thousand feet extent, the several degrees of which have been most assiduously examined by geologists of the first-rate eminence, and upon which the commencement of the history of fishes may be with certainty inscribed at the height of the Ludlow rocks' formation, and possibly even at that of the Dudley rock formation. But, however this may be, we may safely state that it is in the Greywacke group that fishes begin to appear.

These facts are not of a kind in any degree to countenance the ideas which are at present most generally received concerning the succession of organized beings, and the consecutive appearance of animals of the radiata, then the mollusca, the articulata, and the vertebrata, since we find them here commingled together. Their progressive development, on the contrary, presents phases peculiar to each of these classes, and is expressed by various metamorphoses, to which each of them is subjected in their specific characters, and in their mutual arrangements.

Of the genus *Eurynotus*, the following British species are described :—1. *E. crenatus*, found by Professor Jameson in the limestone slate of the Burntisland district, and also in the limestone of Burdiehouse, by Dr Hibbert. 2. *E. fimbriatus*, in the clay-ironstone of Wardie, where it was noticed by Lord Greenock. Of the genus *Platysomus*, the following British species are described :—1. *P. striatus,* in the English magnesian limestone. 2. *P. macrurus*, English magnesian limestone. 3. *P. parvus*, magnesian limestone of Low-Pallion, Northumberland. Of the genus *Gyrolepis*, the following British species are described :—

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* Mr Murchison now names the third formation simply *Cardoc Limestone*, from Caer Cardoc, where it is well seen, and the fourth simply *Llandeilo formation.* These four formations of Murchison, interposed between the English old red sandstone and the transition rocks of the Wernerian school, as described by Jameson, are considered as forming a grand group, and named *Silurian,* from the Silures, the ancient inhabitants of the district where these rocks occur.—Edit.

1. *G. Albertii*, at Wickwar, near Bristol. 2. *G. tenuistriatus*, in osseous breccia at Wickwar. 3. *G. maximus*, at Wickwar? 4. *G. giganteus*. The immense scales, found by Professor Jameson and others in the old red sandstone of Fifeshire and Angus-shire, belong to this species. Of the genus *Dipedius*, the following are British species:—1. *D. politus*, a species characteristic of the lias of Lyme-Regis: Then follows geological observations on the contemporaneous formation of masses of strata, p. 188. 2. *D. granulatus*, the rarest species of the genus found at Lyme-Regis. 3. *D. punctatus* is peculiar to the lias of Lyme-Regis. 4. *D. Colei*, Lyme-Regis. Of the genus *Tetragonolepis*, the following British species are described:—1. *T. confluens*, in the fine collection of Lord Cole, from Lyme-Regis. 2. *T. speciosus*, in collection of Lord Cole, from Lyme-Regis.

But we must conclude, for the present, our extracts from Agassiz' work, with the following curious observations connected with the genera Cephalaspis, Dipterus, Osteolepis, Acanthoides, Cheiracanthus, Cheirolepis, Amblypterus, Gyrolepis, Palæoniscus, Platysomus, and Eurynotus, in all of which the tail is *unsymmetric*, having the one lobe longer than the other.

There is one very remarkable fact concerning the relation of these genera with the geological formations which they characterize; it is this, that all the known species, without exception, have been found in the formations which are anterior to the formation of the lias. Nor can this circumstance be accidental, it is again observed, within the same kind of limits, and upon almost an equal number of species in the Sauridian family; at the same time that all the fish of the Placoidean order, which accompany them in the same formations, have also the same kind of structure of the tail. Some unknown condition of existence, therefore, must have been operating in those remote times upon the development of organic life, and must have effected a conformation which is at once so peculiar and so general: For we are not permitted to regard phenomena so constant as these as simple exceptions, since Nature, in her doings, never admits them on so extended a scale. We can only regard these forms as necessary antecedents of those which have followed; and the traits which characterize and distinguish them as the differences in the progressive developments. These differences consist especially in the transition of an unsymmetric structure to a structure more and more perfect in its symmetry, which has gradually prevailed in subsequent epochs, in which the unsymmetric forms have successively disappeared. To attempt to point out the causes of such a state of things, would be to endeavour to fathom the motives of the Almighty Creator; yet at the same time we may venture to offer some conjectures concerning the relations of the form of these fishes with the external world in which they were destined to live.

If we regard the whole organized beings which have lived simultaneously with the Heterocercic Lepidoidians, or those with unsymmetric tails, we shall remark that they were for the most part fixed at the bottom of the waters, or at least that they crawled about there, without being able to elevate themselves freely and at their wills towards the surface, and move about at large. With the exception of some reptiles, the appearance of which on the earth was much posterior to that of fishes, all these animals were *aquatic*, and the soil bore only those plants which are analogous to those of great archipelagoes or of low plains. Fish, therefore, were the first of the animals to which the power was given of spontaneously gliding through space in all directions, in water; whilst the movements of the Crustacea were only irregular and imperfect. Among the Mollusca, the Cephalopodes, which have the most power of motion, ascended to the surface of the waters, and remained, the sport of the winds in their aquatic ascensions; the Gasteropodes were still more bound to the soil, and the Acephales and Brachiopodes were frequently fixed to it. All the Polypi and Crinoidians of that period appear to have been attached by their base to different solid bodies. At the same time, even the fishes, with their unsymmetrical tails, could not execute the accurate movements of the symmetrical fishes of the following epoch, and their progressive movements therefore would be still irregular. The whole of these animals respiring by branchiæ, could as yet utter no cry, and lived in the most absolute silence. A long period assuredly elapsed ere the surface of the earth was replenished with birds and beasts, and ere man could reflect upon the events which have produced these changes in organic life. We can scarcely conceive that, surrounded with such facts as these, it would be possible to doubt that there was a regular order of succession, and a constant progression in the work of creation.

List of Plants new to the British Flora, or rare in Scotland, observed during the last Twelve Months, in various Excursions from Edinburgh. By Dr GRAHAM.

CERTAINLY there has not been in Edinburgh at any period a greater, I believe there has never been at any one time so great, a number of arduous cultivators of botany as at present. The result of their labours in the field, greatly assisted by my friends Dr Dewar at Dunfermline, and the Rev. A. Robertson junior at Inverkeithing, enables me to communicate the following list, which, I persuade myself, will be received with interest by the botanical readers of this Journal. The whole have been collected within, or scarcely exceeding, a day's walk from Edinburgh, with the exception of those found by Mr Gilbert Macnab near Montrose, and those which were found during the annual excursion which I make with my pupils and some friends in the beginning of August. A desire to attend the meeting of the British Association at Dublin, led us this year into Galloway, and limited the excursion to four days, during which we hastily examined the coast on both sides of the south-western extremity of Scotland.

Acinos vulgaris.—*Roman Camp*, Dalkeith; *Dirleton Common*, abundantly, where, I believe, it was first observed by Mr Stark; *Sandy fields, Arbroath Common,* frequent. Mr Gilbert Macnab.

Allium arenarium.—*Shore of the Frith of Forth near Donibristle.* It has been known for some years to grow here sparingly, but this summer it has been found in considerable abundance. *Pitreavie*, near Dunfermline, where perhaps introduced. Dr Dewar.

Alyssum calycinum.—*Dirleton Common*, profuse, Mr James Macaulay. *Sandy fields on the coast between Arbroath and the Sands of Barry.* Mr G. Macnab. I perfectly know the station in which the species here mentioned was gathered by Mr Macaulay, and even from this alone, without the subsequent confirmation of the discovery by Mr Macnab, I could have no doubt about the propriety of restoring this genus to the British flora, from which it has been erased since the establishment of the genus *Koniga.*

Alopecurus agrestis.—*Among rubbish near Alloa.* Mr Brand. *Sandy field near Gossford.* Mr Stark. Mr Brand believed this to have been introduced with ballast when he found it last September; but the station in which it was subsequently observed by Mr Stark is less objectionable. I do not know any other Scotch station.

Anthriscus Cerefolium.—*Shore near Caroline Park.* Mr W. H. Campbell. Since this was first observed, I have repeatedly visited the spot. It has certainly been introduced with rubbish from the garden, but now thrives luxuriantly.

Bromus arvensis.—*Field at the end of Duddingston Loch*, abundant. Mr Campbell and Mr Brand. This is the first station observed for this plant near Edinburgh. I have since seen it on the side of *Braid Hill.*

Bromus rigidus.—*St David's.* Mr Wallich. This is certainly an introduction upon the ballast heaps, and has no title to a place in the British flora. I have seen the plant in the station where Mr Wallich first gathered it.

Carex extensa.—*Damp spots in the steep bank overhanging the coast on the west side of the Mull of Galloway. On the coast near Usan,* about two miles from Montrose, very abundant. Mr G. Macnab. In the former of these stations, Mr Macnab saw it last year, and this season we saw it in plenty. I believe the only other Scotch station is that on the Fife coast opposite to Edinburgh.

Carex filiformis.—*Beguise Loch,* Forfarshire. Dr Balfour. Mr G. Macnab.

Carex limosa.—*Otterston Loch*, Fife. *Marshy ground between Rescobie and Beguise*, Forfarshire. Dr Balfour. Mr G. Macnab. The former of these stations, in which I first observed it this summer, is the only station for this plant near Edinburgh.

Centunculus minimus.—*Moor behind the Hill of Guthrie*, Forfarshire. Mr G. Macnab.

Corallorhiza innata.—*Near Dunfermline.* Dr Dewar and the Rev. A. Robertson.

Diplotaxis muralis.—*Coast of the Frith of Forth above Queensferry.* Dr Dewar. In this station, which Dr Dewar shewed to me, I certainly think the plant had been introduced with ballast, but he has since found it in a less exceptionable station, farther inland.

Doronicum plantagineum.—*Morlit.* Dr Dewar. *Dalhousie Woods*, Mr James Macnab.

Echium vulgare, variety with flowers striped pink and white.—*Abundant on the sides of a road near North Queensferry.* A variety with flowers entirely white. *Arbroath Common.* The former of these varieties I have seen from *Arthur Seat.*

Epimedium alpinum.—*Bandrum.* Dr Dewar. Introduced. Mr Arnott, I believe, gathered it at *Cleish*, which is in the same neighbourhood, some years ago.

Equisetum Drummondii.—*Woodcockdale Woods.* Mr Campbell. On the 7th of May last, when in company with Mr Campbell, I observed young barren fronds of the plant so considered by him, on the moist wooded bank opposite to the house of Muiravonside; and as he frequently visited that neighbourhood, I requested that he would attend to it as the season advanced. The situation in which he found it immediately adjoins.

Equisetum variegatum.—*Links of Dunbarnie*, Frith of Forth. Mr Horsbrugh.

Erigeron acris.—*Arbroath Common.* Mr G. Macnab.

Ervum tetraspermum.—*Ballast heaps, St David's.* Rev. A. Robertson. *Among debris in a very rocky bay between Auchmithie and Seaton-house, and about half-way between Red Head and Arbroath*, very abundant. Mr G. Macnab. These are the only Scotch stations I know for this plant. Into the first I think it has certainly been introduced,—the last I have not seen.

Fedia dentata.—*Fields between Queensferry and Inverkeithing*, abundant. *Fields between Arborath and Montrose*, very abundant. Mr G. Macnab.

Fumaria parviflora. Now found in *several situations near Edinburgh*, and in some very abundantly. There is great confusion among the characters assigned to the plants considered varieties of F. officinalis and F. parviflora, as I understand them. We have them all growing near Edinburgh, and I think they may be easily distinguished into three species. Of the two forms which I conceive to be described as F. parviflora, that with the larger blossom, the recurved calyx segments, and the lively green colour of its broader leaves, is by much the more common with us.

Gnaphalium margaritaceum.—*Banks of the Yarrow*, Selkirkshire, among willows, about three miles below St Mary's Loch. Mr Thomson. The plant is probably as well entitled to be considered wild here, as in any other British station. I have not been on the ground, but Mr Thomson informs me that there is no garden higher up the stream, nor on the sides of the lake, and that there is not any near the plant, except where the cottagers have raised a few vegetables. The specimens which Mr Thomson has been good enough to give me, are very like such as would be produced in a natural situation.

Helminthia echioides.—*Side of path between Roslin and Lasswade.* Mr James Macaulay found this plant here last July. I cannot help suspecting that it may have been introduced. If otherwise, it is the only Scotch station.

Hieracium molle.—*Near Inverkeithing.* Rev. A. Robertson. I have not seen any specimen, and insert this species on the authority of Mr Campbell.

Iberis amara.—*Inchcolm.*

Iris fœtidissima.—*Near the Pentland Hills.* Mr Graves. *Fordel Woods.* Rev. A. Robertson. I have not seen either station, but I doubt whether they entitle us to place the plant in the Flora Scotica.

Juncus maritimus.—*Near West Tarbet,* Galloway. Mr G. Macnab, who observed it there last year. We found it in the same spot this season.

Lamium maculatum.— *Wood on the estate of Carnock.* Dr Dewar, who thinks the situation a good one. I have not seen it.

Lamium rugosum.—*Banks of the Esk,* towards Musselburgh. *Wood near Torry.* Dr Dewar. In the former station I found it some years ago, and suspected it had probably been introduced. I have not seen the latter station, but Dr Dewar thinks the plant wild.

Lepidium ruderale.—*Fields near St David's.* Rev. A. Robertson. I have not seen the station, but should be afraid to consider any scarce plant wild near that infected district.

Lysimachia nummularia.—*Side of a corn field, Slateford.* Mr Thomson. I have not seen this plant wild in Scotland, and consider the above station too near tainted ground to be relied upon. Mr Mackay told me it is common near Dublin: I did not see it.

Lysimachia thyrsiflora.—*Canal between Edinburgh and Glasgow,* in many places very abundant.

Medicago maculata.—*Donibristle Park. Coast between Donibristle and Aberdour,* very abundant. In both these stations, this plant, new to the Flora Scotica, was first observed by the Rev. A. Robertson. In the former, I certainly believed it had been introduced with grass seeds, but in the latter, which I have not seen, it is believed to be truly wild.

Mentha gentilis.—*Banks of Esk below Hawthorndean,* plentiful. Mr Campbell.

Mentha Piperita.—*Sides of a stream, the origin of the Brothwick, near a spring called Nine Wells, about six miles from Arbroath,* "abundant, and undoubtedly wild." Mr G. Macnab.

Ononis ramosissima.—*Rubbish heaps near St David's.* Rev. A. Robertson. Certainly introduced with ballast. Found in flower in autumn 1834, but has disappeared since.

Ononis reclinata.—*Among the debris at the foot of the cliff overhanging the rocky shore on the west side of the Mull of Galloway.* I found this plant on the 5th of August, both in flower and in fruit, and growing in abundance for some distance in the station described. Ballast is here out of the question. No vessel ever left ballast on that iron bound coast without leaving her ribs also. It is undoubtedly wild.

Ophioglossum vulgatum.—*Dalmeny Woods.* Mr James Macnab. This plant, which is not common in Scotland, and was considered very rare near Edinburgh, was found to be profuse in the above station by Mr James Macnab several years ago, and has been gathered every season since.

Orchis pyramidalis.—*Frith of Forth on the Fife side.* Dr Knapp. *East side of the Mull of Galloway.* Dr Graham. *Near Portpatrick.* Dr Balfour. Last year Dr Knapp added this plant to the Flora Scotica. It was found by Dr Balfour and myself this season.

Osmunda regalis.—*Near Culross.* Rev. A. Robertson. In Scotland, this plant had only been found on the west coast, till last year, when it was first observed in the above station.

Polypogon Monspeliensis.—*Ballast heaps, St David's.* Mr Wallich. Certainly introduced.

Potamogeton prælongus.—*River Lunan, near the Bay, and in Rescobie Loch, Forfarshire. Near Kirkcaldy.* In all these stations, it has been observed by Mr Gilbert Macnab. I have seen it in other places, and believe it is not uncommon.

Primula elatior, variety with single-flowered scapes and umbels growing from the same root.—*Neighbourhood of North Queensferry.* Rev. A. Robertson. I have found this interesting variety, first sent to me by Mr Robertson, in considerable quantity, along several miles of the coast. By far the most common species of Primula for many miles along both sides of the Frith of Forth is P. veris. It is most profuse, and here and there P. vulgaris occurs. It is only where the latter appears, as far as I have been able to observe, that we have P. elatior, so that I feel assured it ought to be considered a modification of P. vulgaris. Whether this modification arises from the introduction of pollen from P. veris, is another question. I think it occurs without this cause, and my opinion is formed partly from the appearance of the variety which I now notice, where the ordinary forms of P. vulgaris and P. elatior grow from the same root (though this is scarcely consistent with the idea of the organization being altered by an admixture of kinds), and likewise from observing that the difference of form which constitutes the distinction betwixt P. vulgaris and P. elatior, occurs among native specimens of P. Scotica, though in the habitat of that species there is no plant by which it could possibly be fertilized, so as to produce a mule, differing in no respect whatever from the specific form, except in the suppression of the common footstalk. The observations of Professor Henslow confirm this view, and that of Mr Talbot, seems to me to be easily reconciled with it.

Ranunculus lingua.—*Otterston Loch,* Fife. *Beguise Loch,* Forfarshire. Dr Balfour. Mr G. Macnab.

Rottbollia incurvata.—*Ballast heaps, St David's.* Mr Wallich. The slender, erect form (R. filiformis) has long been known in several stations on the Frith of Forth. Mr Wallich picked the recurved form; afterwards I received very fine specimens from Mr Robertson, and since then I have seen it at the station. I believe it to have been introduced.

Saxifraga Hirculus.—*About three miles beyond Blackshiels.* Mr Lloyd. This is the most northern station in Britain for this plant, and was discovered by Mr Lloyd while this sheet was in the press. It is distant from Edinburgh in a straight line about twenty miles, and from the only other known station in Scotland about ten miles, the ridge of the Lammermuir Hills intervening.

Senecio Saracenicus.—*Banks of the Almond, not far from Cramond Bridge.*

Scirpus Savii.—*Mull of Galloway.* (After leaving Dublin, I saw this plant in vast abundance in Wicklow.)

Sedum rupestre.—*Banks of Loch Naw.*

Silene Anglica.—*Near Dirleton,* pretty abundant. Mr Stark. *N. Queensferry,* scarce. *Sandy field between the coast and Sands of Barry,* Forfarshire, very abundant. Mr G. Macnab.

Silene conica.—*Dirleton Common, and sandy fields in the neighbourhood,* profuse. Mr Stark.

Silene noctiflora.—*Fields near Dirleton,* common. Mr Stark.

Trientalis Europæa.—*Townhill Wood,* Dunfermline. Dr Dewar. This is the station nearest to Edinburgh in which it has been found abundantly. Much nearer, near the Old Reservoir above Colinton; and at Ravelrig, it was found in very sparing quantity by Mr Edgeworth and Mr Parnell. It has been known in Stirlingshire for several years in two stations very little elevated.

Vicia lutea.—This plant, till lately, was only observed near North Queensferry, in one spot. Mr Robertson has this year pointed out to us a variety of stations in the same neighbourhood, where it grows most luxuriantly.

THE BRITISH ASSOCIATION AT DUBLIN, IN AUGUST 1835.*

President—The Rev. BARTHOLOMEW LLOYD, D.D., Provost of Trinity College, &c. &c.

Vice-Presidents—Lord OXMANTOWN; Rev. WILLIAM WHEWELL.

General Secretary—Rev. W. VERNON HARCOURT, F. R. S., F. G. S.

Treasurer—JOHN TAYLOR, Esq. F. R. S., F. G. S.

Assistant Secretary—WILLIAM PHILLIPS, Esq. F. R. S., F. G. S., &c.

Local Secretaries—WILLIAM ROWAN HAMILTON, Astronomer-Royal of Ireland; Rev. HUMPHRY LLOYD, Professor of Natural Philosophy.

Local Treasurer—THOMAS HENRY ORPEN, M. D.

Monday, 10*th August.*—The different Sections having been organised on the preceding Saturday, the business commenced in each of them this day at eleven o'clock.

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* Report prepared from original communications, and accounts in the Athenæum, Records of Science, and Literary Gazette.

SECTION A.—MATHEMATICS AND GENERAL PHYSICS.

President—Rev. Dr ROBINSON.
Vice-Presidents—Sir THOMAS BRISBANE ; Mr BAILY.
Secretaries—Professor HAMILTON ; Professor WHEATSTONE.

Committee—Prof. Mosely. Mr Whewell. Prof. Lloyd. Dr Drummond. Dr Knight. Mr Murphy. Mr G. Rennie. Prof. Stevelly. Mr Cooper. Mr Wharton. Dr Lloyd. Mr Maccullach. Mr Sadleir. Mr Fox. Mr Snow Harris. Prof. Powell. Dr Dalton. Lord Adare. Lord Oxmantown. Capt. Sabine. Prof. Babbage. Dr Lardner. Col. Colby. Sir J. Franklin. Capt. James Ross.

Monday, 1*st Aug.*—1. Mr Whewell read his report on the State of our Knowledge respecting the application of Mathematical and Dynamical Principles to Magnetism, Electricity, Heat, &c.

He observed that Newton's anticipation that all physical forces are to be explained by some application of the principles of *dynamics*, is verified by the subsequent progress of science, and that the only legitimate theories of heat, electricity, magnetism, &c., are such as are founded on numerical results, reduced to laws. This has been, in a great measure, accomplished; and, therefore, mechanical reasoning is applicable.

He then compared the two theories of electricity, noticing the application of the higher analysis by Poisson, Mr Snow Harris's valuable results, Barlow's Laws of Magnetism, and Bonnycastle's Mathematical Theory, which agrees with experiment and with Poisson's Theory, applied to magnetism.

He appeared to consider that theory untenable, which supposes the existence of one fluid only. He brought forward examples of the distribution of the electric fluid on the surfaces of solids of different forms, which were readily accounted for, on the theory which assumes the existence of two fluids. In connexion with this subject, allusions were made to some statements and experiments of Mr Harris, presented to a former meeting of the Association. Mr W. shewed that similar objections were made to that magnetic theory which required the admission of one fluid only, but that these were not applicable to a theory founded on the supposition of two fluids.

Mr Harris explained, and endeavoured to shew that there is no law for the diffusion of the electric fluid on surfaces of different forms, or of different sizes. He illustrated his position by reference to the loaded Leyden jar, which receives exactly the same charge, whether it is in part filled with a metallic body, or is simply coated with a film of metal on its inside. He also pointed out the fact, that the quantity, or size, or shape of the metallic rods outside the jar, has no influence on the charge which is found concentrated on that part of its surface which is nearest the glass. Mr H. applied the same principles to explain the nature of the charge on air. Several members expressed their opinions for and against both views. It was admitted that Coulomb's theory of diffusion is true in a perfectly imaginary case, but that, in practice, it never can be exactly true, on account of the pressure of surrounding bodies, all of which exercise a greater or less influence in concentrating the electric fluid on that portion of the surface which is opposite to each.

The charge of the intervening plate, it was observed, was independent of the conductors, and the amount of electricity in a body cannot be ascertained but by bringing another body towards it. Hence the results are uncertain. Mr Whewell replied, that the same results were obtained when the connecting body was different; and, therefore, the experiment was not liable to uncertainty.

2. Mr S. Harris explained and exhibited to the Section a new species of instrument of extreme delicacy, for measuring electrical forces. It is of the same kind as the torsion balance of Coulomb, and similar to it in general appearance, but the peculiarity is this:—instead of a needle suspended by a single wire or fibre, which fibre undergoes the torsion against which the antagonist force of the electrical repulsion is to act, the needle is here suspended by *two parallel* fibres run together, one on each side of its centre; and the effect may be most shortly described, by saying, that neither of the fibres undergo torsion, but the *plane* (in which they both lie when at rest) *undergoes torsion*, or is twisted into a surface of double curvature. The degree to which this is carried, is here counterbalanced by a weight; or gravity thus constitutes the antagonist force, instead of the resistance of the fibre to torsion, in Coulomb's construction. It is difficult to give the contrivance a good descriptive name, since

that of torsion-balance cannot be applied, but of its extreme sensibility and accuracy no doubt can be entertained. There are also several very ingenious improvements in the mode of arranging the indices and reading off from the scale, but these are independent of the peculiar and beautiful principle above described, and could not be rendered intelligible but in detail. It is to be hoped the author will give such details to the public.

3. Professor Powell read an abstract on certain points connected with the recent discoveries relative to radiant heat.

The object of his communication was to state, that the author felt particular satisfaction in observing, that M. Melloni (in his second Memoir), describes a repetition of the experiment originally made by him, and recorded in the *Phil. Trans.* for 1825 with perfect success, by means of his extremely delicate apparatus. The confirmation is the more complete, as M. Melloni appears to take up the inquiry with a different object.

It is thus now established beyond question, that luminous hot bodies are sending out two distinct sorts of heat, or two distinct heating agents at the same time, differing in their properties and mode of operation.

Hence, the whole series of results of M. Melloni must be interpreted with reference to this distinction; and possibly the consideration of it may remove some of the apparent anomalies.

And further, if the distinction which he established in the case of luminous hot bodies do really continue to exist (as it seems it must do unless there be a breach of the law of continuity) at temperatures below that of *visible* luminosity (itself a very undefined point), then it ought to be rendered manifest by a careful repetition of the same fundamental experiment, with very delicate thermoscopes (either Melloni's or such as that used by Dr Hudson), by which a smaller, though probably very minute *difference of ratio*, would be found in the effects on a black and white thermoscope, with and without a glass screen.

Should this be found to be the case, in all cases of reflection, polarization, &c. with non-luminous sources, it will henceforth become necessary to distinguish to which of the two heating causes they belong; every analogy would lead us to suppose they belong essentially to that of the two heating agents which is so closely associated with the light, that it seems rather a property of the light than any separate agent. The length of the undulations which Professor Forbes has shewn may by analogy be calculated, would belong to this species of radiant heat, and be simply such undulations in the etherial medium as are too large to affect our visual organs; whilst the other portion may very probably be, as Sir J. Leslie conjectured, merely a conveyance of heat by the air; this would agree with what is observed of its rapidly diminishing as the distance increases, and being incapable of permeating screens except by conduction; this, perhaps, may account for the irregularities found by Sir J. Leslie, in the position of the forces of a reflector for heat; whereas the other sort is conveyed unimpaired to the same distances as the light, and only excited when it is absorbed. A vacuum absolutely perfect, demonstrably, can never be formed with the air-pump, and the Torrecellian, even if free from air and the vapour of mercury, is not large enough for satisfactory experiments, but even if it should be contended that the radiation through vacuum has been proved, we must now say *to which sort* of heat it applies. By this distinction the *theories* of Leslie, De la Roche, and others, may very probably be brought into accordance, since they were perhaps not always speaking of the same thing when they spoke of radiant heat.

Another question of importance which has occurred to the author is this:—Whether, in the polarization apparatus, supposing one glass, or pile of mica, heated, it will radiate the same quantity of heat to the other in the two rectangular positions? The question is purely a mathematical one, and has been in some degree considered at the author's suggestion by Mr Murphy of Cambridge; the integration has not been completed, but Mr Murphy thinks it clear that there will be a difference.

4. Dr H Hudson read a paper on the phenomena usually classed under the denomination of radiation of heat.

He exhibited experiments with the reflecting and differential thermometer, to illustrate the radiating powers of surfaces, interposing occasionally screens with plain and coated surfaces. He tried the diathermancy of rock salt, and shewed several experiments with Melloni's thermo-multiplier, in which the effects were much smaller than those of Melloni.

5. Captain Sir John Ross detailed a new theory of the aurora

borealis, founded on seventeen years' observation. He stated that his attention was directed to the phenomenon by the late Dr Wollaston, who considered that it was beyond the terrestrial atmosphere; but having observed in his first expedition, that sometimes the aurora appeared between the two ships, and also between the ships and the icebergs, he considered it unquestionably proved, that it could not be at that beyond the atmosphere of the earth. Both at his observatory in Scotland, and during his late and long protracted residence in the Arctic Regions, his attention had been particularly directed to this subject, and his conclusions are, that the splendid phenomenon called the aurora is entirely occasioned by the action of the sun's rays upon the vast body of ice, and snowy plains and mountains which surround the poles.

6. Mr Robert Mallet *on an Economic Application of Electric Magnetic Force to manufacturing purposes.* The object in this proposal is, to accomplish the separation of iron filings from other materials, such as brass and copper, in engine-manufactories, iron-foundries, &c. by means of bars rendered magnetic by electro-induction, and deprived of it by breaking the connexion.

Tuesday, 11*th August.*—A communication was read from Colonel Colby, respecting the Ordnance Survey, accompanied with a specimen of the grand national work now in progress.

7. In the Physical Section, Professor Whewell read an important report on *Heat.* He reduced his subject to four heads—1*st*, The experimental evidence of the principles of the doctrine of heat. *2d,* An examination of certain difficulties which affect the fundamental equations. *3d,* The mathematical principles upon which their equations have been discussed; and, 4*th*, The application of these mathematical results to several subjects of practice and speculation. Under the first head, he began with Newton, as the earliest experimental investigator, and gave the history of the law determined by him, viz. that when the temperature of a body exceeds that of the surrounding medium, if the times of cooling be taken in arithmetical progression, the reductions of temperature proceed by a geometrical series. It was soon found that this law was not exact, as it failed to give exact results when the temperature was very high, the rate of cooling

being then faster than the law would indicate. It was also soon discovered that cold radiated as well as heat. The Professor then entered into a most interesting examination of the labours of Dulong and Petit; of Fourier, Poisson, and Cochet. He eulogized justly the sagacity with which Laplace and Biot first saw the difficulty, that the cooling of bodies depended in part upon this conducting power, while the heat was traversing the internal parts, as upon the radiating power, while thrown from the surface. This difficulty was solved by Fourier, who found that the quantity of heat thus conducted from particle to particle within the body, varied as the difference of temperature of the two directly, and inversely as the distance at which they lay from each other. He also showed that the radiating intensity at the surface of a body, varied as the sine of the angle in which that ray came to the surface. Professor Whewell then discussed the second head, but any attempt to give a clear view of the formulæ required for the mathematical discussion of the question would be impossible within the limits to which we must confine ourselves; and we the less regret this, as the report will be printed at length in the next volume of the reports of the British Association. Under the third general head, he entered upon an interesting comparison of the observed and calculated results, and showed the harmony that prevailed between them. Under the fourth general head, he considered—"the application of these speculations to practically interesting cases." Amongst the numerous interesting topics discussed, the following may be selected. The sun, from day to day, is pouring upon the earth a quantity of heat; this, as it descends, by the conducting powers of the parts of the earth, follows certain laws of increase and decrease; and the entire quantity of each year descends to a certain depth, where it is succeeded by the quantity thrown upon the earth in the preceding year, which had not yet been dissipated; below that lies the stratum occupied by the solar heat of the preceding year, and so on, until at length, at a certain depth, this solar heat ceases to be perceptible. He showed, that the mean annual quantity of this solar heat was such as would melt fourteen metres of ice encircling the entire surface of the earth. He next considered the central heat of the earth, and the experiments and observations by which its existence was placed beyond doubt, and the law of its distribution, as it ascended to the surface, traced; and he stated, that the issue from the surface at each part was so much in a century as would be capable of melting three metres of ice heaped upon that surface. He then discussed the subject of cosmical heat—and showed the probability that the regions of space were not of a uniform temperature; hence he concluded, that all the bodies of the solar system had a tendency to acquire the temperature of that part of space in which they are placed, and that the heat of the planetary spaces was only about 50° below the freezing point.

8. Dr Allman, Professor of Botany, next read a very interesting paper on a mathematical inquiry into the *forms of the cells of plants*. His views were illustrated by specimens of plants, as well as by models of the geometrical solids; to us, one of the most interesting portions of this ingenious communication was that part in which he developed the mutual relations of the five solid forms, which, taken two and two, he denominated reciprocal solids, or such as, by slicing off the angles of one, would lead to the bounding planes of the other. Thus, the tetrahedron is reciprocal to the tetrahedron alone, for the angles being equally sliced off, you will, by the slicing planes, be led to another body, contained by the three slicing or truncating triangular planes—the angles of the reciprocal corresponding to the sides of that to which it is reciprocal. The cube and the octohedron he also showed to be reciprocal; and, lastly, the dodecahedron and icosahedron. This was the only part of the paper which came properly under the consideration of the Mathematical and Physical Section. The other very curious and interesting observations on the effect of compression in determining the forms of the cells of plants, were considered as rather coming within the province of the botanist and physiologist than of the mathematician; and, as much business yet remained for the day, Dr Allman, at the request of the President, contented himself with stating some general views in conclusion.

9. Dr Reid, of Edinburgh, read a paper upon Sound, particularly in relation to the precautions necessary to be attend-

ed to in the constructing of public buildings, in which lectures, sermons, or other discourses, were intended to be delivered. He drew the attention of the Section to two distinct cases, first, where, as in a church, the speaker is, for the most part, placed in one fixed place; second, where, as in the Houses of Lords and of Commons, the speakers require to be heard from many various quarters. The author then gave some remarkable instances of the great distances at which sounds had been heard; one of the most curious of which instances was, that when the fleet engaged, we believe, in the blockade of Copenhagen, were in a very extended line, ships at the one end distinctly heard, and recorded in their logs, a loud cannonade, which they heard on a particular day; and it was found afterwards, by a comparison of the logs of the ships, that this very cannonade proceeded from the proving of large pieces of ordnance, which had been conducted for the greater part of a day at a dockyard in the neighbourhood of one end of the fleet, from which the other end, at which the reports were heard, was distant three hundred miles. He also mentioned several well authenticated cases, where the human voice had been heard distinctly at a distance of between one and two miles, and stated, that in numerous experiments made in the open air, under every variety of circumstances, in the neighbourhood of Edinburgh, in which he was assisted by a number of different individuals, they found no difficulty, when the atmosphere was still, of carrying on a conversation at distances varying from 200 to 1000 feet. Dr Reid concluded, accordingly, that where there is any difficulty in the communication of sound in large buildings, this must arise generally, not so much from any want of power in the voice of the speaker, as from a prolonged reverberation maintained between the walls, or between the floor and the roof, and wherever this was observed, there would be no deficiency of sound, or rather noise, but a great want of all purity of tone or distinctness of articulation. He then shewed how the reverberating of sound from the ceiling, walls, and floor of a room, by being continued too long, and interfering with each other, would have the effect of producing a confusing noise, and thus interfering with the hearing of the succeeding parts of the discourse. From all the premises which he had previously laid down, he concluded, that low roofs, having a good reflecting power, rough and interrupted walls, and a floor either possessing very little resilience, such as earthen floors do, or, if boarded, then much broken and interrupted by irregular seating and matting, produced a building best suited to the hearing of a speaker in many directions. He exhibited a plan of his own chemical laboratory in Edinburgh, in which these particulars were all exemplified; and he declared that a speaker, expressing himself in a tone very little above a whisper, could be heard in the most remote parts of that room, whether crowded or empty. The space not occupied by furnace arrangements can accommodate about a thousand persons. The power of the voice being proportioned to the intensity of the sonorous impulse, the area through which it has to diffuse itself, and the manner in which it is strengthened by reflection, all superfluous space ought to be avoided as much as possible, and the direct sound of the voice having been strengthened by one reflection from the roof, it ought then to be entirely absorbed, by being made to fall upon some non-reflecting surface. Dr Reid then alluded to several public buildings in Dublin, where practical illustrations of the views he had explained might be obtained; and, after a number of observations upon the form, walls, roof, ornamenting, &c. of public buildings, and the manner in which many might be much improved, alluded to the influence of different strata of air in the same apartment, arising from imperfect ventilation, or from an equal distribution of heat, which he shewed had considerable effect in some peculiar situations in preventing the free and equal communication of sound.

10. Mr Snow Harris *on the nature of electrical attraction, with experiments.* The author is of opinion, that neither the theory of one nor of two fluids explains all the phenomena of electrical attraction and repulsion. He makes exceptions to the law of the inverse square of the distance, as much depends upon the state and circumstances of the bodies as evinced by delicate experiments. He exhibited his apparatus for measuring the force of attraction between electrified plates and spheres. Some observations made by Professor Powell and Mr Whewell shewed that plates will attract nearly as the inverse distance of the molecules attract in the inverse square. Mr S. Harris explained, and remarked, that it was not his object to refute the theory of

Melloni, or the law of the inverse square of the distance, but that his views were perfectly consistent with facts, and enabled him beforehand to estimate the degree of the attractive focus acting on substances of all shapes, at all distances, by conduced and induced electric action. Professor Stevelly made some remarks to reconcile theory with experiment.

11. Mr J. S. Russel gave a short account of his experimental researches into the laws of the motions of floating bodies. He detailed experiments on two canals in Scotland. He shewed that small models are not applicable for exhibiting the resistance of large vessels, the distinction between the conditions of a floating and an immersed body, with the differences from any theory, and that resistance diminished with great velocities. He exhibited a table of results, and deduced from it that the difference in different canals, according to size and depth, is dependent on the velocity of the wave raised. A member stated that the results are confirmed on two canals in Ireland, but gave a somewhat different explanation. Another member confirmed the results.

Wednesday, 12th August.—12. Mr Pritchard exhibited some experiments on polarization this morning before the meeting; they consisted chiefly of improvements in the mode of exhibition. The most singular fact shewn was, the formation of a certain white elliptic ring, by light passing through calcareous spar.

13. Mr Snow Harris, *on the use of the proof, plain, and torsion balance of Coulomb and others.* He detected considerable differences in the distribution of electricity in different parts of bodies, and obtained results apparently at variance with Poisson's theory. He compared various theories, and exhibited experiments in support of his views. Mr Whewell made some remarks on the experiments of Mr Harris, and on the utility of theories to assign causes. He differed from Mr Harris as to the inferences from the experiments, and shewed, that many of the supposed exceptions are really quite accordant with the mathematical theory.

14. Captain Sabine gave an account of Hansteen's researches on terrestrial magnetism, who had collected observations on the

variation of the needle in different parts of the globe, and demonstrated that there are two magnetic poles in each hemisphere. He exhibited maps of the variation at different dates, and shewed that the line of no dip was not a great circle, but inflected. He noticed the excursion of Hansteen to determine the magnetic poles and variation in Siberia, the observations of Captain Ross and others; and concluded by remarking the absence of any data for determining the position of the southern pole.

15. Professor Wheatstone examined with a prism the line of light formed by the voltaic spark between charcoal points. The spectrum was complete without interruption, but marked by certain lines of more intense brightness. With the electric light from a surface of mercury, a few very bright lines of definite colour appeared separated by wide dark intervals. The same light from different metallic surfaces gave similarly interrupted spectra; the divisions being distributed in a peculiar manner in the spectra for each metal. In alloys and compounds of different metals, the lines peculiar to each wire were compounded. The voltaic light from a surface of mercury, and of other metals in succession, gave analogous results. At different points of the connecting wire the light appeared of different colour; that colour was found marked with a peculiarly bright line in the corresponding spectrum. High characteristics afford the means of distinguishing the different metals, and to determine the origin of the electric light in different cases. The light from *combustion* of metals gave uninterrupted spectra; thus shewing, that voltaic light is not related to combustion. The light of the *electric* spark from contact of different metals gives similar light and dark spaces to those in the voltaic. Mr Wheatstone applies these results to the theories of electric light, and thinks they point out the volatilization of ponderable matter from the conductors, as the most probable hypothesis.

16. The Rev. Mr M'Gauley exhibited and explained a new principle, consisting in the application of magnetism as a moving power. The apparatus consists of two powerful electromagnets, so arranged that the contact is alternately changed from one pole to the other, and the poles of the magnet thus reversed. This produces an alternate motion in the bar connect-

ing the poles, which moving a crank turns a wheel and any machinery connected with it. The inventor has succeeded in thus getting a moving power nearly equivalent to the lifting power of the magnet. The main peculiarities may be thus stated:—1*st*, The reversing apparatus easily applied, without almost sensibly altering its weight, to any number of electro-magnets. 2*d*, The connexion of the reversing apparatus with the vibrating bar. 3*d*, The direct application of the entire, or nearly the entire, lifting power to machinery. 4*th*, The action of several electro-magnets, without their interfering with each other.

17. Mr R. W. Fox *on a new Dipping Needle.* The instrument which shews, at once, the dip, intensity, and variation, was exhibited. It contains graduated circles, moving in altitude and azimuth, and a vertical index adjusted to the meridian, turning in azimuth till the needle is vertical. Hence the dip may be deduced, and the intensity, by comparing the effect of a small magnet, introduced.

Professors Stevelly and Lloyd made some remarks. The accuracy of the instrument was confirmed by the testimony of Sir John Franklin, who had examined it in connexion with Messrs Christie and Barlow, even under extreme conditions.

18. Professor Hamilton *on the theory of Logologues, and other numbers of higher orders.* He discussed the nature of algebra, and whether it is to be considered a science, or only a language? His leading idea is, that it may be regarded as the science of pure and abstract time, as geometry is the science of pure space; and that the distinction of positive and negative corresponds to past and future. He regards imaginary quantities as couples of moments, or couples of steps: a given moment as having a given position, in regard to time, as a given point in space: each step has direction and magnitude. He referred to his paper in the Irish Transactions, which contained his suggestions for certain improvements and simplifications in algebra.

Thursday, 13*th August.*—19. Mr R. Roberts exhibited an apparatus by which figures may be seen, or sentences read, as distinctly when revolving at a high velocity, as when in a state of quiescence. It illustrates, also, the duration of light on the eye.

20. Mr Baily read his report on the Aberdeen standard scale of 3 feet, which was compared with that of the Astronomical Society, by the recommendation of the Association. He found a small difference, and concluded with some suggestions for the better construction of such scales. Sir Thomas Brisbane remarked the necessity of attending to expansion. Professor Stevelly made some observations on the same point, to which Mr Baily replied, by stating the precautions which had been used.

21. Mr Snow Harris *on Thermometric Observations.* He produced a report of hourly observations, conducted at Plymouth, and illustrated it by diagrams, representing the mean daily curves of temperature at different periods of the year. He compared them with similar observations made at Leith.

22. Mr G. Jerrard *On the Solution of Algebraic Equations.* An account of these researches was given by Professor Hamilton.

23. Professor Phillips presented the third report on the fall of rain, in continuation of his former reports. It fully confirmed his remarkable results announced last year to the Association, viz. that less rain falls, in proportion to the height from the ground; and extended them so as to lead to some general theoretical views. Mr Phillips proposed that the Association should either provide, or recommend members to procure rain-gauges, and have them placed at different places, at different heights from the ground, not to exceed 150 feet, which he appeared to think quite sufficient for the collection of data, to enable calculations to be made to ascertain the quantity of rain which would fall into a gauge at any height. This suggestion led to some discussion in relation to the dew which would be collected in the gauge along with the rain, and thus would vitiate the result. It was also stated, that radiation from different places had a great influence on the formation of vapour. Thus, the Sugar-loaf and Howth mountains are frequently capped with clouds, when the Dublin mountains are quite clear; hence the necessity of care being taken in placing the gauges. It was also stated, that there is a quantity of rain discharged from a variety of those clouds known by the name of *nimbus*, which never extends to within a considerable distance of the ground. These clouds, which do not extend to the ground, are well known. The rain

falls into the lower air, and is absorbed by it; the latent heat for the solution being furnished by the lower stratum of air, or by radiation or reflection from the ground itself. The gauges, arranged as proposed, would be no measure whatever of rain of this kind, and would consequently furnish erroneous results for calculation. The discussion was discontinued, in consequence of the press of business.

Professor Rigaud made some observations.

24. Colonel Sykes read various details as to the mode of conducting observations on temperatures, as practised by himself in India, with a general statement of his results. His method was to determine the boiling point of water, at the altitude he wished to fix. In several examples the error was only 16 feet in 4000. The apparatus recommended was very simple, cheap, and portable.

25. Mr M'Cullagh *on reflection and refraction of polarized light at the surfaces of crystals.* This communication consisted of the results of a mathematical investigation of certain questions relative to the polarization of light, which arise on comparing the theories of Fresnel and Cauchy, and was full of most important views.

26. Mr M'Gauley *on Magnetism.* This paper was a continuation of his former communication, in which the author entered at large into his theory of electro-magnetism, contending that it is merely a state of electric excitation, rejecting the theory of currents, and concluding with observations in favour of free inquiry on philosophical subjects.

Friday, 14*th August.*—27. Professor Apjohn on the *Dew Point.* The author propounded a formula of connexion between the dew point and the indication of the wet bulb thermometer. His results tended to shew the conformity of observations and theory.

28. Professor Hamilton read an abstract of Mr Challis's results respecting the simultaneous vibrations of a cylindrical tube, and the column of air contained in it. In the theory of cylindrical tubes, it is necessary to take into account the vibration of the tube itself, as well as the air contained. Certain laws have been deduced. It is probable that the vibrations of the pipes

severally affect the vibrations of the columns of air, and consequently the sound produced.

29. Professor Wheatstone *on Speaking Machines*. The author noticed the attempts made by the Germans to form pipes which would imitate the vowel sounds. De Kemplin imitated different sounds with a conical tube, more or less covered at the mouth. The author observed, that the difference of the vowel sounds depended solely on the relative size of the cavity of the mouth, altered by the tongue and the opening of the lips. Willis, by a tube (containing a reed), altered in capacity by sliding, extended the scale to other vowel sounds. He proposed a new mode of classifying the consonant sounds. He exhibited a copy of the machine of De Kemplin, in which the mouth is imitated by an India rubber bell, and consonant sounds produced.

30. Mr Whewell *on a new Anemometer*. The object kept in view in the construction of this instrument, is to measure the variation of intensity corresponding to the time. The instrument was exhibited. It consists of a small vane, with sails like a windmill, turning to the wind, and round its axis. Its circular motion is thus converted into a much slower vertical motion. A pencil is moved which traces a line on a paper cylinder, shewing the direction and strength of the wind. Sir John Ross made some remarks on his own method of judging of the intensity of wind at sea, and of the means adopted by him, during his last voyage, to register the direction of the winds, and their velocity, combined with the state of the weather, barometer, and thermometer, from observations as accurately made as circumstances would admit of, every half hour. He expressed his doubts of the utility of the instrument in the Arctic regions; but said that it would have been desirable to have had such an instrument with him in his last voyage, in order to test its utility and accuracy.

31. Professor Lloyd detailed his magnetic observations in Ireland, made in connexion with Captain Sabine, which were undertaken at the request of the Association. They consisted of observations of the dip and force. The lines of equal dip were pointed out, with irregular variations of intensity, probably depending on atmospheric causes not yet understood. The ines of equal intensity were in some measure determined.

These have been found not generally parallel with lines of equal dip; but in Ireland they are not greatly different from parallel. At the south-east point of the county of Waterford, the least dip was observed; the greatest was on the north-west point of Mayo, or Donegal. In Limerick, the dip was 71°; in Dublin it was less. It increased towards the north-west, in the direction of the west magnetic pole, 71½°. In Armagh it was about 71·15½°. The author remarked the utility of having magnetic observations made at different stations. An interchange of needles was made between Captain James Ross and Captain Sabine, by which their respective observations would be made to correspond.

32. Mr Hamilton *on the Theory of Varying Orbits.* He explained the general nature of orbits, under the influence of perturbations, expressed by Lagrange as a varying ellipse. The author's method is a modification of this, by different ellipses, founded on a former general method, in a somewhat simplified form.

Mr M'Cullagh mentioned some investigations of his own upon the same subject; and stated, verbally, a rule which he had deduced.

Professor Powel stated certain difficulties of calculation which had occurred in his optical researches, with a view to elicit any assistance which the members of the Association might be able to give in the solution of them.

33. Mr Kane *on the Interference of Sound.* He referred to Herschel's suggestion of the distinction of sound by interference, as analogous to the absorption of light. In trying the experiment he found anomalies in certain cases.

Mr Addams remarked that the anomalies might easily be explained, by considering the actual conditions of the case.

Mr Wheatstone made some remarks, and described experiments which seemed to reconcile the anomalies alleged.

MECHANICAL SCIENCE APPLIED TO THE ARTS.

The First Meeting was held on Thursday 13th August.

President.—Mr RENNIE.
Vice-President.—Dr LARDNER.

1. Mr Hodgkinson of Manchester detailed some experiments in reference to the collision of beams and piles. This communication formed the continuation of a former paper read to the Association. The results were, 1, That when cast-iron beams were brought forcibly in contact with balls of different kinds of metals of equal weights, the deflection was the same in distance, whatever the nature of the metals was. 2, That the impinging masses rebounded after the stroke, through the same distance under similar circumstances. 3, These effects are not in any way dependent on elasticity, but are the same as theory would point out to be the consequence of the collision of bodies destitute of elasticity. He mentioned also that wire resisted fracture most effectually, when it was extended by means of a weight, equivalent to one-third of the weight required to break it.

2. Mr Mallet read a paper on the fracture of bars of cast-iron.

3. Mr Ettrick read an account of a compass which, by a peculiar contrivance, adjusted the cardinal points so as to correspond with the true points in the horizon; thus obviating the necessity of allowing for the variation. This effect was produced by securing the needle upon the card by moveable clamps, and adjusting it for the magnetic variation of Greenwich, with a contrivance for altering it in situations where the local variation was different.

4. Mr Pritchard shewed an achromatic microscope, in which the angular aperture of the object-glasses exceeds any that have hitherto been constructed. It is peculiarly serviceable in examining flax, cotton, silk, &c.

5. Mr Russell read a paper on the solids of least resistance, in reference to steam-vessels, and related experiments which tended to prove that the form of prow best suited to rapid progress through water, was a parabolic one. This was disputed by Professor Mosely and Dr Lardner.

6. Mr John Taylor stated, that by calculating from the duty on steam-engines in Cornwall, it appeared, that work performed by means of one bushel of coal, required ten or twelve years ago two bushels; and in the time of Bolton and Watt's patent four bushels were necessary: while, at the commencement of the use of steam power, the coal required was sixteen bushels. The steam engines at present in Cornwall were equal to 44,000 horse power.

7. Dr Lardner made some observations on rail-roads. He stated that every road offers a sensible resistance to traction, but this on a rail-road is less, because the surface is more uniform. The resistance on a rail-road to the power of traction is always the same, as the resistance produced by ascending an acclivity, rising one foot in 250; that is, supposing the rail-road to be level. Suppose a rail-road rising 1 foot in 250, resistance to traction then proceeds from two causes,—the resistance on the level, as already explained, and the resistance offered from the actual declivity. The resistance to be overcome on the level is equivalent to nine pounds per ton; and on the road ascending 1 foot in 250, it would be eighteen pounds per ton; and thus it is seen that, in the latter case, the drawing power must exert twice the force necessary on the level. If the road rose 2 feet in 250, the drawing force would be twenty-seven pounds to the ton. This view of the subject is confined to ascents, but it should not be forgotten, that when a rail-road is worked, the transit is from one end to the other. It is necessary, in estimating the merits of rail-roads, to consider their action downwards as well as upwards. In coming down a steep, no force is required to impel an engine, and the gravity restores that force in going down which it has robbed from it in the ascent. You have to provide in an ascent of 1 foot in 250, for a resistance of eighteen pounds to a ton, but in descending, no force is required. If it was desired to strike an average between the ascent and descent, the road would present a surface which would be equivalent to a level. This point, respecting ascent and descent, struck the House of Lords, before which he gave this opinion, as a paradox, but it was one only in sound and not in reality. Dr Lardner remarked, that these observations referred to ascents not more steep than 1 foot in 250; but supposing the rise to be 3 feet in 250, and where the strain would be, consequently, thirty-six

pounds in each ton, would gravity give this back in the descent? It was true that no power was required in descending, but while only nine pounds were gained in the descent, twenty-seven pounds were lost in the ascent. Beside the loss of power, there was also the danger resulting from the too great velocity occasioned by sudden descents. In one of the lines of railway, for which a bill had been applied to the House of Lords, there was a slope of 1 foot in 106, in a descent of two miles and a half long, and the velocity given to an engine on arriving at the foot of the slope could not amount to less than seventy miles an hour. To mitigate defects arising from these abrupt descents, breaks were applied, but not always with success. The break is a piece of wood, pressed against the tire of the wheel by a lever, and if it acts with full effect, it ought to prevent acceleration. He had seen several cases in which it had totally failed, and one instance which occurred, he would detail. At one of the slopes between Manchester and Liverpool, he was descending with a loaded train of 150 tons. The operative engineer, whether through a desire of displaying the engine's movements, or through neglect, forgot to apply the break at the commencement of the slope; when half-way down, the velocity became so great, that he requested the breaks to be applied, but on doing so, they were instantly burned. The train went down at a tremendous speed, although the supply of steam had been cut off. When the train had been stopped, it was found that the wheels of one of the waggons which revolved with the axis, had been broken, and yet, notwithstanding this accidental drag, the speed amounted to at least fifty miles. It was objectionable to have any slope exceeding 1 in 250, for when the excessive natural powers of gravitation were resorted to, control over its movements was impossible. The conclusion to be arrived at, although it appears paradoxical, is, that you may construct two rail-roads, say of 100 miles in length, one level, the other going over mountains, and yet the two rail-roads may be worked by the same mechanical power. Suppose in the one you ascend 1 in 250, and descend in the same ratio, a pull of eighteen pounds to the ton is required only fifty miles, and on the other half you descend by inertion. On the level road, a pull of nine pounds to the ton is required, from the entire distance of miles, and thus the extent of exertion is equalized. It was not, however, to be forgotten, that they should have a regard to the power used. If the power to be used was that of animals, then it might happen that the hilly road would be better than the level; for nothing was better understood than that a dead and unvarying pull upon the same set of muscles, would have the effect of causing the labour to be more severe, while a varying pull would alternately give quiescence and exercise to the muscles. If the line was so disposed as to throw the whole ascent in one pots, the advantage would be gained of having the rest of the road nearly level. But the cost of attaining this advantage should not be forgotten. Steeps of this description required an increased power, and the engines capable of working on the general line of road would not be capable of exerting an increased force. There were only two ways of ascending sudden ascents, one by the agency of an additional engine, and the other by having the whole train pulled up by means of a rope. The additional engine would occasion much additional expense, for the supply of them would always be preserved, and the men should be paid their wages whether wanting or not. The use of the rope would occasion an enormous waste of power; and he mentioned the instance of a place, where an ascent of 1 foot in 106 occurred. The rope was five miles long, and its weight was 60,000 pounds. Dr Lardner next referred to one point on which he seemed to consider that engines generally were at variance with what was correct. He contended, that the heat of the fire is directly proportional to the quantity of the steam allowed to escape in a definite time into the flue, and consequently that half the number of blasts of steam projected into the chimney in an engine going up a hill, would have the same effect in exciting the fire as double the number of blasts of half the condensation, when the engine was running on a level plane.

Friday, 8*th August.*—8. Mr Ettrick read an account of certain improvements on steam-engines, and on securing the steams of boilers, of a machine for drilling boiler plates, and of an astronomical clock.

9. Mr Cheverton read a communication on the sculpture of busts by machinery, exhibiting beautiful specimens.

10. Mr Grubb made some very able and practical observa-

tions on an improved model for mounting an equatorial instrument, adopted by Edward J. Cooper, M. P., of Macroe Castle, county Sligo. Mr Cooper bore testimony to the excellence of the instrument, as also to the talent and perseverance of Mr Grubb in his scientific improvements and inventions.

11. Lieutenant Denham stated some details in reference to the vibratory effects of rail-roads, especially over tunnels. A discussion then took place between Dr Lardner and Mr Vignolles, on the disadvantages arising from acclivities on rail-roads, which appeared to the Section as over-rated by Dr Lardner.

12. Mr Stevelly described a self-registering barometer. The principle of this instrument is to suspend the mercurial cistern by a mercurial hydrometer, so that it may move downwards by arithmetical distances, for equal additions to its weight, and *vice versa*. Hence, if a scale be fixed beside it, an index carried by the cistern as it falls and rises will mark the corresponding positions.

SECTION B.—GEOLOGY AND GEOGRAPHY.

President.—Mr GRIFFITH.

Vice-Presidents.—Mr MURCHISON. Professor SEDGWICK.

Secretaries.—Captain PORTLOCK. THOMAS JAMESON TORRIE, Esq.

Committee.—G. B. Greenough, Esq. James Bryce, Esq. Major Clerke. William Smith, Esq. John Taylor, Esq. Sir G. Mackenzie. Dr Traill. Henry Woolcombe, Esq. M. Agassiz. William Nicol, Esq. Rev. W. Turner. Sir A. Crichton. Sir John Franklin. Sir John Ross. Captain Sabine. Lord Cole. Colonel Colby. Thomas Larcom, Esq. R. Taylor, Esq. J. Taylor jun. Esq. R. Fox, Esq. Elias Hall, Esq. James Macadam, Esq. Rev. Sydney Smith. Captain James Ross, R. N. Dr Scoular. A. Nolan, Esq. Viscount Adare. Lieutenant Stotherd. Captain Denham, R. N. Dr West. Thomas Botfield, Esq. J. Menteath, Esq. Colonel H. Montgomery. James Smith, Esq. Lieut. Berdes. Joshua Trimmer, Esq. Williamson Peile, Esq. C. König, Esq. Decimus Burton, Esq. J. Preswick, Esq. M. C. F. de Montalembert. M. Ed. de Verneuil. Samuel Turner, Esq. Sir James Douglas.

Monday, August 10.—1. Mr Griffith presented his Geological Map of Ireland, the result of many years' research and labour; and pointed out the difficulties which at present attended the delineation of the boundaries of the formations consequent on the imperfections of the existing maps. He said he of course excepted the maps published by the Ordnance survey, than which nothing could be more splendid. The map in the present case adopted is Arrowsmith's, and it had been found necessary to make the geological colouring conform to the geographical inaccuracies it contains. As an example of such imperfections, it was mentioned, that, in Arrowsmith's map, Ben Wee Head, which is due west of Sligo, is placed twenty miles north of that parallel. A new map of Ireland is about to appear in London, and which, if it shall be found to agree with the trigonometrical survey maps, it is Mr Griffith's intention to employ. In pointing out the remarkable features of the country, Mr Griffith dwelt on the position of the mountain groups, which, except when limestone occurs on the coast, form the margin of the island, and inclose a vast plain which is chiefly of limestone. Owing to this arrangement the river courses are short, except that of the Shannon, which is 140 miles long, and falls 80 feet the first 20 miles, and only 80 the remaining 120 miles. On the great plain of the interior there occur extensive ridges of gravel, called "escars," which are sometimes 100 feet in height, and which, though nearly constant in direction when considered in small spaces, are variable when the comparison extends to spaces of greater magnitude. They run for ten, twenty, or even thirty miles, and formerly the roads were frequently constructed on their summits. In the county of Mayo, the gravel hills run east and west, whereas to the north they run north and south. According to Mr Griffith, the great bogs were formed in hollows, in which the water was dammed up by these gravel hills. Under the bogs very extensive and deep deposits of marl occur underlaid by clay and gravel. The marl is in some instances forty feet in thickness. The colours adopted by Mr Griffith in the construction of his map are chiefly those used by Mr Greenough. Confining himself, on this occasion, to the stratified rocks, Mr Griffith first considered the gneiss and slate districts, and stated his opinion that the primary group of the north-west and west of Ireland corresponds to a similar group in the Grampians of Scotland; while the Down slaty group is probably connected with the rocks of Dumfriesshire, &c. No relation has hitherto been ascertained to exist between the Wicklow series and any group on the opposite side of the Channel. The general direction of the stratification to the north of Ireland is north-east and south-west, but in some parts of Tyrone it is more north and south. In the south the direction approaches more to the east and west. At Dunmorehead, county of Donegal, the quartz-rock exhibits a concretionary structure similar to that of orbicular granite, and is often mistaken for a trap-rock. The limestone beds associated with the schists of Donegal are not continuous, though they occur in certain lines. They are sometimes cut off by trap-dikes, and then become dolomitic. The primary district of Mayo and Galway is similar to that of Derry and Donegal. Mr Griffith proposes to divide the transition slates of Ireland into groups, as soon as the state of the science enables him to do so. In the newer portion of these deposits occur alternations of sandstone and clayslate; and again a limestone similar to that of the Blackwater, and containing organic remains, alternates with the upper part of the schist; and, as in this latter case the limestone contains fossils of the mountain limestone, the schist which alternates with it must also be of comparatively recent origin. A section from Dungarvon to Carrick was exhibited, and another in Waterford. The old red sandstone is considered by Mr Griffith as divisible into two or even three parts, viz. 1*st*, The common old red; 2*d*, A white variety which occupies a central position; and, 3*d*, A sandstone which alternates with the oldest part of the coal formation. In the county of Mayo there are beds of the sandstone alternating with limestone. Though there seems to be a transition from the limestone to the sandstone, yet there are always portions of the formations where in the one all the strata are of limestone, and in the other of sandstone. The mountain limestone formation extends from the county of Cork to Fermanagh, and occupies two-thirds of Ireland. A section from Newcastle in county Down to Benbulbin in Sligo, was exhibited. A singular cavern occurs in the limestone under the mountain of Kulkeagh. It contains no stalactites or stalagmites, but its sides have a remarkably smooth and polished surface. Mr Griffith regards the calp of Kirwan as the lowest bed of the mountain limestone, but as a member of the series which ought to be distinguished from the series of Benbulbin. The calp rarely if ever contains fossils. Mr Griffith next described the various coal-fields of Ireland, beginning with those of the south, and particularly that of Cork. There the coal strata consist of *stone-coal*, shale, clay-ironstone, fire-clay, a quartzose rock which can hardly be termed sandstone. The dip is to the south, and the limestone at the edge of the coal-field dips to north, but the exact point of junction at Blackwater is not visible. The coal strata have an undulating character. A section of parts of the counties of Cork and Limerick was exhibited. The coal formations of the north contain *bituminous coal.* That of the county of Monaghan rests on greywacke, and its lower beds are chiefly sandstone; and that of southern Tyrone and Fermanagh contains thin seams of coal. The coal-field of northern Tyrone, which is richer, and has been described in a separate report by Mr Griffith, consists of two parts, an outer of limestone, sandstone, and shale, and a little coal; and an inner, containing a greater proportion of coal, which is of a soft description. There is another coal-field in the north-west of Fermanagh, and there is also the singular and disturbed coal district of Antrim. The formations newer than the coal strata occurring in Ireland, are confined to the north-east part of the island, and consist of new red sandstone, a magnesian limestone, lias, greensand, and chalk. The new red sandstone occurs on both sides of the trap district of Derry and Antrim, viz. to the west, beginning at Caledon, and extending due north to Macgilligan, county of Derry, whence it may be traced round to Donegal; and to the east of the trap district it extends past Belfast. On the south side of Belfast loch there is a deposit of magnesian limestone, the only one in Ireland. In some places the new red sandstone abounds in gypsum. The lias occurs at Larne, in the county of Antrim. Some details were given respecting the greensand and the chalk. Mr Griffith deferred the consideration of the unstratified rocks till another day.

Mr James Bryce jun. of Belfast, offered some remarks on Mr Griffith's account of the Geology of Ireland. He stated that the north of Ireland presents many evidences of diluvial action, in the form and direction of its valleys and gravel ridges, in the loose masses of rock which everywhere strew the surface, and in the contents of the deposits of gravel which are common over the whole country. The county of Down is composed of granite and greywacke, Antrim and eastern Derry of the trap formation, Armagh and Tyrone of sandstones and the carboniferous series, while western Derry and Donegal are mostly primary. No district could therefore be conceived more favourable for observing the phenomena of transportation; and we accordingly find that there is a marked distinction in the nature of the detritus in different parts. Over all the basaltic area it consists chiefly of various kinds of trap, as we should naturally expect; but mixed with it there are fragments of rock which could only have come from the west, and several of which have actually been identified with the rocks of Donegal and Western Derry. Passing into the latter district, we find in the gravel beds no fragments of the rocks of Antrim or Eastern Derry, all their contents still point to a western origin. The fragments are of much larger size along the western border of the trap district than towards the centre or eastern part, proving that the currents diminished in force as they rolled to the eastward. If this transport took place subsequent to the formation of the Derry mountains, we must admit that the currents were strong enough to carry large blocks up inclined planes of a height varying from 300 to 1500 feet. On the transition tract of the county of Down, the most abundant rock in the detritus is of course greywacke, but there occur also rolled masses of trap in great plenty, of chalk, of chalk flints, and greensand, so far to the south-east as Portaferry and Downpatrick, these being rocks which could only have come from the trap district, which is well known to be every where encircled by chalk and greensand, lias and sandstone. In the north-east angle of Down, large pieces of chalk, with its characteristic fossils, are frequent; and seem to point to the *formation of Belfast being at a period subsequent to the transport of these masses.* In the diluvial ridges in the valley of the Lagan, the detritus consists of the secondary neptunian and trap rocks of Antrim and Derry, and many varieties of primary rocks: no greywacke is found there, though it exists over all the country to the *south* and *south-west;* nor have we any indications of the limestone of Fermanagh and Armagh, which lies to the west, and the whole phenomena seem clearly to point to a *north-west* current in these parts, and, therefore, to the *posterior origin* of Lough Neagh, as it lies exactly in that direction, and as we cannot suppose that any current could possess sufficient force to transport rocky materials through so deep a basin. The direction of the diluvial ridges over all the area under consideration, with the exception of a few local variations, which can be easily explained, also attests the action of currents from the west and north-west. Mr Bryce next spoke of the magnesian limestone of Cultra, near Hollywood, in the county of Down, which he remarked occurs within a space about a mile long and a few inches broad. It is fine grained and compact, and is generally of a grey or bluish-grey colour. It alternates in thin layers with strata of sandstone, which there can be no doubt is the lower portion of the new red sandstone series. A bluish-black shale, of a soft and friable texture, and unctuous feel, but destitute of bituminous matter, is associated with the sandstone in some places, and seems to be inferior to the limestone; it effervesces feebly with acids. The beds of sandstone below the limestone are often conglomerated in their structure, and are interstratified with a conglomerate containing fragments of quartz, greywacke, with a yellow earthy and a grey crystalline limestone imbedded in a slightly calcareous base. To these greywacke slate succeeds. The conglomerate and sandstone below the limestone agree very closely in character with the calcareous conglomerate of the west of England, and the "inferior red sandstone" of Professor Sedgwick, which underlies the magnesian limestone of the east of the same country, and which that distinguished geologist has identified with the "Rothe Todliegende" of Germany. The calcareous blue shale alluded to seems beyond doubt the equivalent of the "marl slate" or "kupferschiefer," the compact bluish-grey limestone to hold the place of the "Zechstein," while the upper red sandstones or gypseous marls of the vicinity of Belfast are the types of the "keuper and bunter sandstein," the muschelkalk being absent in Ireland as in England, and the rauchwacke being represented only by a partial and unimportant yellow limestone. Mr Bryce remarked it was a remarkable instance of the uniformity of formations, that there should occur in an insignificant spot, rather less than a mile long and only a few inches broad, a series of beds, which seem the exact equivalents, in relative position and mineral structure, of deposits which occupy in the east of England a space 200 miles long and from ten to forty broad, and on the continent of Europe are still more extensive. Mr Bryce then alluded to several other points connected with the geology of Ireland, and more particularly to the greywacke of the county of Down, which, in his opinion, has been there converted into gneiss, mica-slate, greenstone, chlorite-slate, &c. by the action of the adjacent granite.

Professor Sedgwick noticed the similarity which seems to exist between the fossiliferous greywacke of Cumberland and Wales, and a part of the transition series described by Mr Griffith; and also the resemblance of a portion of what is termed the old red sandstone of Ireland to a part of Mr Murchison's Silurian system. The phenomena of the magnesian limestone of Ireland seem to the Professor identical with those exhibited in England.

Mr Murchison pointed out the resemblance of the Pembrokeshire coal district to some of the Irish coal-fields, and also noticed, as a striking peculiarity in the geological features of Ireland, the alternation of the old red sandstone with the mountain limestone, as described by Mr Griffith. There seems in this respect a greater analogy to the formations of Scotland than of England. In the latter country the line of demarcation is clear and well marked.

Captain Portlock, R. E., observed that a closer examination of facts was necessary, before decided conclusions could be drawn on so important a question as that involved in the direction of those currents or forces which had given rise to the accumulations of gravel and sand so common on the earth's surface, as well as to the dispersion of larger masses or boulders. In Ireland, little dependence can be placed on conclusions drawn from fragments of primitive rocks, since the sources from which they have been derived are to be found in such varied positions. Peculiarities of mineral structure may indeed remove this difficulty, but it is evident that a very rigid and careful comparison must precede any deduction to be drawn from the position of the removed fragment, and that a perfect knowledge of all existing localities of the parent rocks is equally essential. From fragments of chalk and basalt more decided arguments might be deduced, but in the cases which had been cited, viz. those of Down and Antrim, the distance of the gravel beds from the chalk and basalt cliffs is really so small, that they can only be considered evidence of the direction of a local current, and instances are not absolutely wanting of that direction being to the westward. Nor is there any thing surprising in the absence of evidence of a movement in any one locality contrary to the general direction, since the analogy of existing facts would render such an occurrence improbable. In other parts of the island, the varying directions of the ridges tend to shew a similar variation in the direction of the forces which produced them; whilst the great mass of the inland gravel points to a local origin, being composed of pebbles of limestone. It is indeed to the great central plain of Ireland that that observation should especially be directed, as it affords sufficiently varied materials on its borders, and admits of their free dispersion. It must not be imagined that the streams of rivers are here supposed to be the dispersing agent, but the great inequalities of the surface of the globe are fairly believed to have afforded local modifications of the currents sweeping over the surface, and that independent of their origin, whether it be supposed to arise in one great convulsion, or in various and successive local elevations and distributions; and it is also necessary to consider the dispositions of gravel and sand alternating, as they often do, in regular arrangement, as very different phenomena from those scattered blocks which often rest on their surface.

Mr Greenough mentioned the high gratification he had experienced in witnessing the completion of the geological map of Ireland, a work which he had urged Mr Griffith to prepare twenty years ago, and at the same time expressed his admiration of the harmony and distinctness of the colouring. After many experiments, Mr Greenough has arrived at the conclusion, that it is impossible to arrange a series of colours adapted to all countries. Mr Greenough then alluded particularly to Mr Conybeare's paper on the connection of the rocks of Scotland and Ireland, and stated his belief that the Morne mountains and Criffel are nearly identical.

2. Dr West read a paper on errors committed by authors, regarding the geography of some parts of Greenland. The chief object of the memoir was to prove that Cape Farewell, a promontory on the south coast of Greenland, so called by Davis in 1585, (vid. Gatonbe's Journal in Churchhill's Voyages, vol. vi.), is not, as Egede, Crantz, and Giesecke state, on the island of Germesok, but on another many leagues to the south-east of it;

and that Statenhoek is not, as stated in No. 59. of the Edinburgh Review, an inlet, nor yet, as represented in most maps, the most southern extremity of the mainland, but that it is identical with Cape Farewell, and received its name, which means State's Promontory, from the Dutch navigators. The memoir was accompanied by a copy of the southern part of Graah's chart of Greenland, the only accurate one that has yet appeared; from the inspection of which, it was evident that no part of the mainland of Greenland could be seen from the open sea to the south, and that the only island on which is a promontory answering to the description of Cape Farewell, is that most to the south of the sound Ikaresckeoak, a sound considerably to the east of Sermesok, which last has been brought down by Giesecke more than half a degree too much to the south. An extract was also given from Graah's narrative, to shew that he was of the same opinion; as well as one from Drage's account of the voyage in California, published nearly ninety years ago, in which it is expressly stated that Cape Farewell and Statenhoek were the English and Dutch names of the same cape. A singular error in the Edinburgh Gazetteer, and in the index to the translation of Malte-Brun, was adverted to. The error in the placing two Cape Farewells on the south coast of Greenland, and moreover, in making that on the east coast more to the west than that on the west coast, in nearly the same latitude. In conclusion, the author stated his opinion that Lieutenant Graah, by ascertaining that there were no ruins on the east coast of Greenland from its southern extremity to lat. 65¼°, had completely decided the long disputed question respecting the situation of the Œsterbygd or eastern settlement, by establishing the correctness of the supposition of Eggers, that it had been on the south-west coast, and had received its name merely from the circumstance of its lying to the east of the other or vesterbygd.

Remarks highly complimentary to Dr West, were made by Mr Murchison, Sir J. Franklin, and Mr Smith of Jordanhill.

Tuesday, 11*th August*.—3. Archdeacon Verschoyle read a notice of a system of trap-dykes passing through the counties of Mayo and Sligo. The trap-dykes of that district are remarkable for their frequent occurrence, the unusual length and directness of their course, the effects produced on the adjacent rocks, and above all their regular parallelism. The author has examined eleven of these dykes within thirteen English miles measured across their course, which is nearly east and west, varying only 4° or 5° to the north of west. The length of one of these dykes, viz. that extending from Rinvoe Point to the bed of the river Bonnet near Dromahair, county Leitrim, is sixty-three miles, and its breadth is not more than forty feet. Another runs from Broad Haven to Agrishead, a distance of forty-five English miles; and there are several others which have been traced for forty miles.

Mr Griffith spoke of a paper on a similar subject by Lieutenant James, about to appear in the Transactions of the Geological Society of Dublin.

Captain Portlock mentioned that the direction of the dykes in Tyrone is due north and south.

4. Professor Phillips communicated the results of his investigations on fossil Astacidae. He remarked of the species which composed the genus Astacus, as at present existing, some are found in salt and others in fresh water. There are two empirical characters by which the marine may be distinguished from the fluviatile astaci; in the marine species, the lateral divisions of the tail are transversely divided, while in the fresh water species the division is longitudinal. The marine species have also large didactylous claws to the first pair of feet. All the fossil species possess those characters which belong to the marine division. Proceeding to investigate the question as to the possibility of identifying strata by means of their organic remains, it was remarked that the study of the fossil species of the present genus does not afford results very favourable to such a hypothesis. Confining our attention to the oolite and lias, it was observed that one species of astacus was found in every bed, from the lowest of the lias to the uppermost of the oolite. One species was confined to the coral rag; four species were peculiar to the greensand; some of the species were more local, and others appear to have had a wider geographical distribution, as is the case with the astacus of the present day.

Mr Greenough, Professor Sedgwick, and Mr Murchison, complimented Professor Phillips highly on his successful labours, and it was remarked that this paper was one of the advantages derived from the recommendation made to Professor Phillips by the British Association.

The Rev. Sydney Smith exhibited a specimen, belonging to Mr Fox, of an Echinus, from the mountain limestone of the county of Kildare.

After the meeting, the members proceeded to the Ordnance Survey Office, Phœnix Park, where they had an opportunity of examining every part of that splendid establishment, and had the advantage of the explanations of Colonel Colby, and the other officers engaged in the survey.

Wednesday, 12th August.—A copy of the ordnance survey of the parish of Templemore, presented by Colonel Colby, was laid before the Section.

5. Lieutenant Stothed described a small isolated patch of granite in the county of Cavan, surrounded by transition and secondary rocks. The effects of this rock on the greywacke schist near it is such as is usually noticed, the schist passing through several stages of induration, and ultimately assuming the character of quartz rock. The occurrence of this low isolated patch so far removed from the great granitic masses of Down, of Galway, or indeed of any other part of Ireland, is in itself interesting and peculiarly important, as it seems to explain many of the appearances of the interesting tracts of schist, such as their broken and unconnected state in low distinct knolls, and the appearance of undulations they so often exhibit.

6. Mr Griffith concluded his observations in illustration of his geological map, by describing the nature, distribution, and mode of occurrence of the various unstratified rocks which are met with in Ireland. He spoke first of the granite districts. The granite district of *Dublin* and *Wicklow* is fifty miles from north to south, and eleven in breadth. *The district of the Morne Mountains in Down* is characterized by the rocks containing much hornblende, whereas there is none in the Wicklow rocks. A transition may be observed from granite to sienite. The district of Down contains no mica slate, and is in this respect different from Wicklow. Beryls and topazes occur in Down, and beryls have also been found in Wicklow. The strata of Down run N. E. and S. W., and this direction is not

altered by the granite. On the S. and S. W. sides the strata are somewhat distorted, but on all sides the rocks are materially altered at their contact with the granite. The characters of the slates are changed, and the greywacke is converted into a quartz rock. Veins proceed from the granite into the slate, and produce alteration of characters. The district of Antrim consists of mica slate containing granite, which is often porphyritic, and may sometimes be termed porphyry. The masses of granite sometimes resemble beds in the mica slate. Mr Griffith entered into a variety of details regarding the sienites, traps, and porphyries of the north and other parts of Ireland. He seemed to consider that the phenomena he had observed prove that the sienite of the north of Ireland is of posterior origin to the chalk and the porphyry. The granite of Galway and the porphyry of Lowth were described, and some instances given of the conversion of limestone into dolomite at its junction with trap. In Donegal, a point was mentioned where a columnar quartz rock lies between two portions of non-columnar trap. The ochre beds in the island of Magee, &c. and their relation to the porphyries, were then discussed. Many interesting views were exhibited of the coast of the northern trap district. The trap-dykes and horizontal beds of trap were noticed, and their general direction given as follows;—those of Mayo E. and W., of Fermanagh N. of W., of Down S. of E. and N., of Antrim N. and S. A geologically coloured copy of the mining index map of the county of Down was exhibited.

Mr Bryce remarked, that an idea seems to prevail among many geologists that the Giants' Causeway rests upon chalk, but this notion is erroneous. Ballygilly Head, near Larne, is a precipice of columnar basalt, washed by the sea on the east, and resting on sandstone. The chalk abuts against it on the south, the upper surfaces of both rocks being on the same level; the chalk encircles it completely on the western side, and is resumed in the same place as before on the northern side, the basalt being thus isolated in the chalk. At the Causeway the same seems to take place, though the parts are not so clearly shewn. The chalk abuts against the basalt at Port Bradin, both rocks being at the same level, and from that part westwards the whole range of magnificent precipices rests on lias or sandstone. In the interior, however, about a mile south of the Causeway headlands, the chalk seems to continue its course straight west in an unbroken line, till it re-appears near the mouth of the Bush River. In fact the Causeway seems to be merely part of a great ridge or outburst of columnar trap, which runs E. and W. along the north coast, and includes the Fairhead, the southern promontory of Rathlin, the various islands, and some parts of the coast about Ballintoy, the Causeway itself, and the rocky isles adjacent, and finally the Skerries and Ramore Head at Portrush. In its whole course it seems to cut off the upper secondary rocks, and at Portrush to have been infused beneath the lias clay forming the altered basaltiform fossiliferous rock at that place, whose nature has been so much controverted.

7. Professor Phillips communicated the results of his investigations regarding the Geological distribution of Belemnites. He observed that no less than one hundred species were now known, and of these about thirty-four species had been found in England. Shells of this genus are confined to the chalk, oolite, and lias, and the results which their study affords contrasts remarkably with the negative indications deduced from an examination of the fossil Astaci. One division, characterized by a slight swelling at the apex, and possessing a lateral fissure, was confined to the chalk. The species, which were obtusely mucronate, are found in the green-sand. The species with a groove on the back are found in the middle oolite; those with a lateral groove in the lias and lower oolite; and those species which are destitute of a groove are confined to the lias. From these remarks, it appears that not only are the species of Belemnite confined to certain strata, but that even certain natural divisions of the genus are found together in the same beds, and in no others. Another interesting remark is, that species which are common in the chalk of the Continent, are rare in the chalk of England, and *vice versa.* M. Agassiz communicated some interesting observations on the organization of Belemnites, and on their relations to recent *Cephalopodes.* By a careful examination of a specimen in the collection of Miss Philpot, at Lyme Regis, M. Agassiz has convinced himself that the conical body generally termed belemnite, is the termination of the horny plates frequently found at the same locality with the ink bags, and

which have been described as specimens of a fossil Loligo. It results from this fact, that the fibrous cone of Belemnites is analogous to the projecting point of the extremity of the bone of the Sepia, that the plate which has been described as a Loligo corresponds to the external portion of the plate of the bone of the Sepia, and that the alveolar cone of Belemnites represents the internal portion of the *os sepiæ*; so that the genus to which we must refer the Belemnites and the fossil Loligo as parts of the same animal, approaches much the genus Sepia, and, indeed, differs only in the relative proportions of the conical and dilated portions of the shell. Professor Buckland is about to publish, in his Bridgewater Treatise, a representation of the specimen which has enabled M. Agassiz thus to unite these fossils, and he will also give the details which prove the connection. But the history of this genus cannot be regarded as complete till we know also the changes which take place in the form of the belemnitic cone at different ages, so that we may not be led to establish species from differences depending on growth.

8. Lieutenant Denham, R. N., one of the officers attached to the Ordnance Survey of Great Britain, exhibited a map illustrative of the estuaries of the Dee and Mersey. He was one of the first to discover, along with his brother officers, an important channel in the Mersey, after various ineffectual and expensive efforts had been made to improve the navigation of that river, an object of the greatest importance to Liverpool. The channel runs north and south, at right angles to the tide which washes over it. Lieutenant Denham called the attention of the Section to the half-tide level, as a means of determining the level of the sea and the land, the main level, or half tide, being always constant, the high tide being variable from various causes. In illustration of these views upon this subject, he exhibited a chart of the estuary of the Dee and Mersey, and a tide-table, calculated so, that the mariner may know any half hour the banks which might be crossed, thus pointing out what was dangerous, and when it ceased to be so, a matter of great relief to the mariner. Lieutenant Denham complimented the dock-trustees of Liverpool, for their munificent expenditure, in enabling him to prosecute his object, and dwelt with particular energy on the great liberality of Sir John Tobin. Lieutenant

Denham suggested the utility of establishing a half-tide level at each port, to point out the soundings of banks at the half tide along the continuous shore. He next alluded to the error which had been nearly committed, in cutting a canal from Bridgewater to the sea, when there were tides from a fifty feet level to eighteen feet. He observed, that if the distance from the earth's centre to the half-tide level was calculated, it would form a correct base for ascertaining the heights of the land. It would form a matter for consideration, if the influence of the sun and moon on the tides were withdrawn, whether or not the water would recede to the half-tide level. Mr Murchison said, this showed the intimate connexion between geology and geography; and that if this subject was followed up, the encroachments of the sea in one place, and the increase of land in another, and the rate of that increase and decrease, might yet be defined.

Mr Griffith gave an account of a mass of shelly gravel in the county of Wexford: this deposition is very extensive, stretching along the coast for a distance of seventy miles, and attaining a breadth of eighteen. The following is a section of this deposit:

5 feet of clay
7 feet marl clay
7 feet marl
7 feet of sand
11 feet of gravel, containing abundance of marine shells.

Thursday, 13*th August*.—9. Mr Williams read a notice of fossil plants collected in the coal districts situated on the opposite sides of the Bristol Channel, viz. of Devonshire and Pembrokeshire. These remains are in either case most abundant in the shales that divide the seams of anthracite coal or culm, as it is provincially termed; and the object of the author's research was to ascertain whether there was any geological affinity between the two fields of non-bituminous coal. After a careful investigation, Mr Williams arrived at the conclusion that they are of different ages; that of Devon being, in his opinion, a true transition coal contained in a schist, while that of Pembroke and Caermarthen belongs to the carboniferous series proper above the mountain limestone. Specimens of the plants were exhibited, and also some interesting lias fossil fishes.

10. M. Agassiz, in placing before the Section the fifth part of his researches on fossil fishes, and also 112 plates of English ichthyolites newly figured, and which will appear in the succeeding numbers of his work, made some observations on the general results he had obtained from the inspection of the greater part of the collections of Great Britain. One circumstance, which is certainly remarkable, and which gives great consistence to these researches, is, that the discovery of nearly 400 new species of fossil fishes, has in no degree modified the conclusions which the author had deduced from the examination of 500 species, with which he was acquainted, when he commenced the publication of his work. The most important of these results for the geologist is, that very intimate relations in organization exist among fishes of the same epoch, and that progressive changes may be remarked in the fossils of different formations; so that it appears to the author possible to determine the relative age of a deposit, even by the examination of unknown species, and solely by the peculiarities of their organization. M. Agassiz then offered some remarks on the mode of deposition of the beds which contain the fossils, and on the difficulty of reconciling the mineralogical phenomena of stratification, and the conditions of existence in which the fossils contained in these beds must necessarily have been placed. It appears to him probable, that all the fossils contained in a formation have lived together, and perished in consequence of one catastrophe; that the organic remains which have been preserved in these beds, are rarely the debris of animals which have successively perished during epochs of comparative repose, but rather the remains of those which have been suddenly buried, in consequence of revolutions which produced great changes in the *ensemble* of organic beings. It seems to M. Agassiz impossible, that the animals and plants, which appear in *different* layers of the *same* geological formation, could have lived successively on the inferior, middle, and superior beds of that formation, since, after the conditions of their existence, and the conditions under which the beds must have been deposited, can neither be reconciled, nor have alternated, as it would be necessary to admit, in

order to understand this succession; and if these fossils had really lived in all the *ctages* of a formation, it would be difficult to conceive how certain very thin beds of great extent do not contain on their surfaces, at different intervals, evident traces of life, and of the existence of the organised beings, whose debris we find in the inferior strata, as we find, for example, rolled blocks, fragments of wood, &c. The state of conservation of coprolites, which has been advanced as a proof of the existence of saurians, around which they are found, seems, to M. Agassiz, to prove, on the contrary, that these substances have only been deposited during the *agonie* of the animals from which they were derived; for if they had not been enveloped in the strata which contain them, at the very moment when they were deposited by the animal, it would be inexplicable how we should find bodies so soft still entire, even after being a few hours in the water. Whenever evident traces of slow deposition occur, M. Agassiz believes that such beds should be attributed to periods of comparative repose, and not to formations which have had their origin during cataclysms, in consequence of which the earth has assumed another aspect. It would, therefore, be of importance to distinguish more precisely the different effects produced by destructive causes, which have acted at the surface of the earth. These causes seem to M. Agassiz connected with the nature of the earth, and dependent on its particular organization, much rather than on its external relations with other celestial bodies, and their influence at its surface.

Captain Portlock exhibited specimens of a fossil fish from the new red sandstone of a quarry near Roan Hill, Dungorman, parish Killyman, county of Tyrone. M. Agassiz stated, that the fossil belongs to the genus Palæoniscus; but that it constitutes a new species, remarkable from the dorsal and anal fins being placed very far back, and from the dorsal being nearer the anal than in the other species. The form is very elongated. The discovery of this fish is the more interesting, as it extends the geological limits of the genus Palæoniscus, of which the species hitherto known have been found only in the coal formation and in the magnesian limestone. M. Agassiz proposes to give to this new species the name Palæoniscus catopterus. Mr Murchison, after pointing out to the Section how successfully

M. Agassiz had already demonstrated the value of fossil fishes, in marking with precision the age of the rocks in which they are found, showed how ably the Professor had illustrated the different genera and species of fishes in the old red sandstone. Mr M. then passed a warm eulogium on the labours of M. Agassiz, which he characterized as almost unparalleled; and stated that the Professor had just completed a long journey from Neufchatel, by Munich, to be able to procure the illustrations of his last fasciculi, which were now laid upon the table. He congratulated geologists on having the merit of being the first to induce M. Agassiz to visit England, by awarding to him the Wollaston medal; and he still more congratulated the meeting upon the satisfaction they must experience in witnessing the valuable results of the judicious grant of money which had been specially allotted to M. Agassiz at Edinburgh, to enable him to illustrate British fossil ichthyology.

11. Dr Traill read a sketch of the physical geography and geology of Spain, founded on his own observations made during a tour in that country. He confined his remarks chiefly to the province of Andalusia. In this interesting country we have every variety of rock, from the oldest primitive up to the tertiary strata. The mica slate of Andalusia contains many interesting minerals, as iron glance, and lead ore. This last mineral is so abundant, that no less than 35,600 tons were extracted in one year. The primitive rocks are succeeded by secondary limestones, in whose fissures interesting osseous remains occur. These limestones extend to the opposite coasts of Africa. This limestone is followed by new red sandstone, gypsum, and marl, abounding in salt and saline springs. Oolite rocks occur near the ancient town of Cartua; and chalk, with flints, is observed at Labriga. Tertiary limestones also occur, as has been noticed by Colonel Silvertop.

Mr Greenough made some observations on the connection of the Alps and the Pyrenees.

12. Mr Smith of Jordanhill read an account of some fossil trees found in a sandstone quarry near Glasgow, viz. near the aqueduct which carries the Forth and Clyde Canal across the Kelvine river. Arguments derived chiefly from the erect position

of the trunks, were adduced to prove that the trees had grown in the place where they now occur.

After the adjournment of the Section, some of the members proceeded on a geological expedition to Killiney, where they examined an interesting junction of granite and mica slate.

Friday, 14*th August.*—13. Mr Whewell offered some very interesting explanations and remarks on various points in physics connected with geology, and more particularly on the permanency of the half-tide level, on terrestrial magnetism, on Fourier's views regarding central heat, and on the opinions lately expressed in Poisson's Treatise on Heat.

14. Mr Hartop described some remarkable faults in the southern division of the West Riding of Yorkshire, and offered to continue his investigations.

15. Mr Murchison and Professor Sedgwick gave a lucid and most interesting account of their views regarding the older rocks of England and Wales, endeavouring to point out their subdivisions, so as to complete the series from the old red sandstone to the oldest slate rocks of Cumberland. Mr Murchison treated of that part of the subject relating to the deposits which occur between the old red sandstone and the slaty rocks of Wales. This group of rocks has been termed by Mr Murchison the Silurian System, and has been studied by him during the last five years in Herefordshire, Shropshire, Radnorshire, Brecknockshire, Monmouthshire, and Caermarthenshire. The detailed results of these important investigations will soon be given to the public in the work by Mr Murchison, which is now in preparation. Professor Sedgwick explained the conclusions he had deduced from his elaborate and extensive examination of the greywacke, or slate series, of the north of England and Wales. Professor Sedgwick divides the old transition series into three groups, viz. the Lower Cumbrian, consisting chiefly of slates, and containing no organic remains; the Middle Cumbrian, composed of slates, conglomerates, and porphyries, and containing fossils; and the Upper Cumbrian, which contains much limestone, is fossiliferous, and approaches the lower beds of Mr Murchison's Silurian System.

Mr Murchison stated, that he had discovered fossil fishes in the old red sandstone of the district which he has lately examined, which Mr Agassiz has declared to be identical with the ichthyolites of Caithness. Mr M. is of opinion that the Silurian system of rocks exists to a very considerable extent in Norway.

Some very interesting observations on the transition series were made by Mr Greenough and Professor Phillips.

Dr Daubeny requested the co-operation of the Section, in furthering his inquiries into the nature and phenomena of mineral waters.

16. Dr Jacob read a notice regarding some fossil madrepores from the Queen's County.

17. Professor Phillips made a communication regarding the occurrence of a tertiary deposit on the coast of the north of England, near Burlington.

18. Professor Sedgwick read a letter from Mr Lyell on the fossils of the Crag.

Monday, 10*th August.*

SECTION C.—CHEMISTRY AND MINERALOGY.

President.—Dr THOMAS THOMSON.

Vice-Presidents.—Dr DALTON and Dr BARKER.

Secretaries.—Dr APJOHN and Mr JOHNSTON.

Committee.—Mr Davy, Mr Vernon Harcourt, Dr Daubeny, Mr Graham, Mr Connell, Dr R. D. Thomson, Mr Kane, Mr Ferguson, Mr Scanlan, Dr Geoghegan, &c.

The Secretary presented to the Section printed copies of tables, exhibiting at a single view the most important properties of simple and compound bodies, for defraying the expenses of the printing of which L. 10 had been allocated at the last meeting of the Association.

1. A paper was then read by Mr Davy, upon the subject of the corrosion of iron by sea water. The observations had particular reference to the injury sustained by the iron of buoys

subjected to the influence of sea-water in harbours, as at Kingstown; where it was recently found that the rings, upon which the safety and utility of the buoys mainly depend, rapidly corrode and are destroyed. Mr Davy turned his attention to the important object of providing a remedy, and preventing the corrosion of the iron; and although his experiments had only recently been commenced, still he considered it proper to bring the few results he had procured before the Section, for the purpose of exciting further inquiry. He found that zinc applied to iron prevented corrosion. Rings of this metal were cast into forelocks for the purpose of experiment, and were found to obviate the waste to which the iron had previously been subject. According to Sir Humphry Davy, the cause of the corrosion of copper and metals in contact with sea water, is attributable to the access of atmospheric air. He considered that if the air was preserved from coming in contact with the metal, no decomposition would ensue. Mr Davy accordingly found, that copper exposed to the action of sea water, free from the influence of air, was not liable to corrosion, and that the effect was influenced by the depth of water. Specimens of metals were exhibited, which had been subjected to the influence of salt water free from air, and no corrosion had taken place; other pieces of metal which were in contact with sea water, subject to the influence of air, were observed to be much injured. Mr Davy attributed the cause of the phenomenon to an electrical decomposition. He stated further, that he had found zinc to preserve tin-plate, both in fresh and salt water. Some observations were made by members of the Section with regard to the action of sea water upon bar and cast iron. Some attributed the greatest corrosion to the former, others to the latter.

2. Mr Ettrick described an improvement which he had made upon Davy's safety-lamp, for the purpose of obviating accidents which are entirely owing to the carelessness of workmen. The Davy lamp, he stated to be perfect in principle. The workmen are in the habit of enlarging the apertures in the wire gauze, and applying their tobacco pipes in order to obtain a light. The modifications recommended at present were, the introduction of very strong glass, to cover the gauze externally.

The glass is again guarded by strong ribs of iron, so that the lamp may be exposed to considerable shocks without danger of injury. A contrivance was also described by which the air was allowed to enter from below, by means of a gauze tube, but so managed, that the gauze could not be reached by the workmen. Various improvements upon the Davy lamp were noticed by different members. Mr Graham stated, that he had found that when the gauze was steeped in an alkaline solution, the flame was prevented from passing so readily, and corrosion was obviated. He considered the only adequate provision against accident to be the employment of a double gauze cover.

3. Mr Kane read a communication in reference to pyroxylic spirit. The experiments which he had made upon this substance corroborate the opinion of its composition entertained by Dumas and Pelligot, who term it methylene, viz. that it is a compound of an atom of carbydrogen, and 1 atom of water, having for its atomic weight 2.

4. Mr Fox described an experiment which he had made with regard to the effect of melted iron upon the magnet. He found that no action was exerted upon it. Hence this is an argument against the idea of a central fire.

5. A letter was read from Dr Turner, reporting the opinion of the committee appointed at last meeting to take into consideration the adoption of a uniform set of chemical symbols for this country. The opinion of the majority was, that those used on the Continent should be had recourse to. It was strongly recommended that the abbreviations should not be carried further than the dots for oxygen; indeed, it was suggested by some that these should be rejected, as they merely express theory, and, consequently, vary according to the view that is taken of the composition in this country and on the Continent; but it is obvious, that if brevity is not carried any further than this, no bad consequences can follow from a system of notation. Dr Thomas Thomson strongly recommended that the centigrade thermometer should be adopted in this country for scientific purposes, as being infinitely better adapted for such purposes than that of Fahrenheit. His suggestion appeared to coincide exactly with the opinion of the Committee.

Tuesday, 11*th August.*—6. Mr Davy detailed some experiments which he had made upon the preservation of tin-plate by the agency of zinc. When exposed for some days to the action of water, the plate by itself soon becomes slightly corroded, but is completely preserved by the zinc, the latter at the same time oxidizing. Hence, the plate might be employed in place of copper for many purposes, where salt water comes in contact with vessels. Several metals, he had ascertained, are not protected.

7. Mr Graham described the constitution of certain salts, in continuation of the papers which he has published upon this subject.

8. Mr Johnston made some observations on the optical properties of chabasite, in reference to those made by Sir David Brewster at last meeting. Sir David found that this mineral possesses different refracting powers at different depths of the crystal, and he concluded that it consisted of distinct layers; and that, if subjected to experiment, it would afford the result of a compound substance. His results refer to particular species only; but the composition of the species vary, and are as represented here,

$$\left.\begin{matrix}C\\N\\K\end{matrix}\right\}\begin{matrix}S^2\\S^3\end{matrix}+3AS^2+6Aq.$$

where Mr Johnston conceives that it is easy to see the cause of the difference, for the refractive power of chabasite is positive, and that of quartz negative; thus accounting for the double refractive power observed by Brewster. Dr Thomson remarked, that the observations of Brewster probably referred to one species of chabasite, but there are two species, the one containing soda and the other lime as a base. He, therefore, considered that until both species were examined, no inference whatever could be drawn.

9. Dr Daubeny stated, that, according to the opinion of Von Buch, carbonate of magnesia must have been sublimed in many instances by volcanic action, although, as far as Dr Daubeny was aware, it was not agreeable to the results of chemists. A curious fact illustrative of the truth of Von Buch's opinion occurred to Dr Daubeny in Italy. He visited a locality where there was an upper stratum of lava, containing cavities. In one

of these Colonel Robinson discovered a large quantity of carbonate of magnesia. Dr Daubeny found a quantity coating the upper surface of the lava. Dr Dalton observed, that there could be no doubt as to the sublimation of carbonate of magnesia, as Dr Henry had informed him that a quantity of this salt was always driven off whenever the heat was carried beyond a certain height.

10. Dr Dalton stated the results of his examination of the spirit distilled from caoutchouc. He found it to depress the barometer like sulphuric ether. It passes through water without diminishing its volume, thus differing from ether. It is absorbed by water like olefiant gas. It consists of 2 olefiant gas. 10 volumes when burned give 40 carbonic acid, and require 60 of oxygen. It appears to be the same as a substance described by Faraday. It differs from coal-gas in this, that the latter consists of double olefiant gas. The observations of Mr Davy upon this subject corresponded with those of Dr Dalton.

Wednesday, 12*th August.*—11. Mr Mallet described the phenomena presented in lamps, when the holes for the passage of the gas are made as small as possible, and also the appearance observed when the direction of the tube is inclined in different ways, two currents being formed when the tube is inclined, and the surface of the flame presenting spiral lines, and considerable retraction of the flame taking place, none, however, occurring when the tube is not fully inserted. The apertures in the lamp were less than the $\frac{1}{100}$ of an inch in diameter. In the discussion which arose from this communication, Dr Dalton observed, that twelve small holes in a lamp consumed less gas and gave more heat, than when the holes were larger but fewer in number. But the great object in procuring a proper quantity of heat depends upon the atmospheric air being neither too great nor too small in quantity. He stated, that if we take a cubic inch of pure gas, and another diluted with half its volume of air, each gives out the same quantity of heat, but the latter scarcely yields any light. This is an important fact, and deserves to be known.

12. Mr Connell read a paper, in which it was his object to point out some chemical facts, by which we may be enabled to

detect whether a fossil scale be that of a fish or sauroid animal; and illustrated his position by some analyses which he had made on recent crocodile and fish scales, and upon the scales found at Burdiehouse. His inference was, that chemical analysis completely proved that the scales found at Burdiehouse were those of fish, as had been already shewn to be the case, but on other grounds, by Professor Jameson. He considers the animal matter to be replaced by a little carbonate of lime and silica.

13. Mr Kane described two compounds of tin and platinum, formed by the action of protochloride of tin upon a solution of platinum. One of these compounds consists of an atom of each chloride. It deliquesces in the air; is a dark solid substance when anhydrous, and when allowed to remain in the air is converted into an olive liquor, which is resolved into the oxides by action of water. The author suggested that tin affords a good test for platinum.

14. Mr Snow Harris exhibited an apparatus, or modified electrometer, for performing the experiments of Pouillet, by which the insulation of the gold leaves is rendered independent of the glass, by means of two rods, terminating in gilded balls. To determine whether electricity is developed during the evaporation of water or any liquid, a platinum crucible, containing the substance to be examined, is placed upon the cap of the electrometer, having one of Deluc's small piles communicating with the rods. His results were contrary to those of Pouillet.

15. Dr Newbigging made some observations upon an experiment which he had made with regard to the colour of arterial blood. He placed some blood in a cup, containing green spots on its surface. The portions opposite to these spots assumed a vermilion colour, but in no other part was this change visible.

16. Mr Hartop made a communication on the use of the air-blast in the manufacture of pig-iron, in which he shewed that the saving said to be effected by the use of hot air had been overrated, as a considerable portion of the alleged saving had been previously effected by other improved processes. The general saving on the average, he stated to be no more than 10s. per ton, and observed that the price of *such* iron in the market had actually fallen from 15s. to 20s. per ton, while that from cold air at the same time rose 5s. per ton in Yorkshire. This

statement gave rise to observations on the part of several gentlemen who differed from Mr Hartop, as the reduction in the price of iron from hot air had not occurred in other parts of the country, and that, as prepared in Glasgow, and many other places, it had not been so deteriorated. It has, in consequence, been adopted in every smelting-house in Scotland, and the annual produce of the works in that country during the last ten years has been nearly doubled. Reference was also made to processes adopted in the Russian smelting-works, which shewed that, by a judicious adjustment of the quantity of cold air introduced by the blast, a saving could be effected approaching even to that obtained by the use of hot air.

17. A letter was then read from Colonel Colby, accompanied by a copy of the ordnance survey of the county of Londonderry, in the hope that the Section might suggest some improvements, or additional topics of consideration to be introduced into the survey.

18. A paper was then read by Dr Apjohn on a new method of determining the specific heats of the gases. Having arrived at an accurate formula representing the relation between the temperature of the wet-bulb thermometer and dew-point, including the specific heat of air as a coefficient, he was enabled, by observations of the depression in perfectly dry air, and the application of the formula in question, to determine the specific heat, not only of air, but of a variety of other elastic fluids. His results are, that all gases but hydrogen have, under equal volumes, specific heats proportional to their specific gravities; so that, with the exception just mentioned, all have, under equal weights, the same capacity for caloric.

19. Dr Dalton introduced the subject of a system of chemical symbols, by explaining his ideas respecting the composition of the simple compounds, and exhibited the expressions which he proposed many years ago, to give a pictorial view of the mode in which the atoms are collocated. He considers the composition of nitrous oxide to be two atoms azote, adopted by Berzelius, who has not stated from whom he obtained it. Olefiant gas, he considers, is composed of single atoms of carbon and hydrogen, while the gas which exists in coal, though commonly termed olefiant gas, is, in reality, double olefiant gas, and is

termed by Dr Dalton bin-olefiant gas. This is proved by its affording twice the quantity of carbonic acid, and requiring twice the quantity of oxygen to burn it which olefiant gas requires. Mr Whewell observed, that the atoms might as well be supposed to be arranged in lines, as in the mode represented by Dalton, which was objected to by the latter, as being a tottering equilibrium.

20. Mr Mallet shewed a beautiful white material, prepared from turf, which was declared by a paper-maker to be perfectly fitted for the manufacture of paper. The upper stratum of turf, which covers immense tracts in Ireland, consists of layers. It is acted on by water to separate the leaves; then by caustic potash or soda; then by an acid. It is then bleached by chloride of lime. During the process, a substance is obtained possessing the odour of camphor, mixed with that of turpentine, which is fluid at 290° F. The upper stratum of turf may also be employed for mill-boards, after being soaked in glue and pressed by a hydraulic press.

Friday, 14*th August*.—21. Mr Davy described some experiments which he had made in reference to the relative values of Virginian and Irish tobacco. He procured nicotine by simply digesting the leaves in potash, and then distilling. A liquid possessing uniform qualities passed over. The liquid is acted on by acids, affording salts possessing a sharp biting taste. The effect of the liquid was tried upon different animals, and found to be highly narcotic. He found that 1 lb. of Virginian tobacco was equivalent to 2⅓ of Irish tobacco; the root containing 4 or 5 per cent. of nicotine. The usual estimate of the relative values, by dealers, is as 1 to 2.

22. Mr Scanlan detailed the experiments which he had made upon what he considered a new fluid, prepared from pyroligneous acid by saturation with lime, distillation and purification by charcoal. He found its boiling point to remain steady at 130°. The following table exhibits its peculiarities when compared with pyro-acetic and pyroxylic spirits.

	Sp. Gr.	Boiling Point.
Pyro-acetic, ...	.828	150°
Pyroxylic, ...	.750	140°
New Fluid, ...	.906	130

Another fluid was obtained likewise, which appeared to be new, exhibiting a strong action with caustic potash. It was suggested, that the first fluid was acetate of methylene, the specific gravity of which is .919, and the boiling point 136°. The arguments of Mr Scanlan were admitted by the Section to be conclusive in favour of the substance being distinct from pyroacetic, or pyroxylic spirits.

23. Mr Moor mentioned a curious circumstance in reference to the corrosion of lead pipes. The worm of a still used for preparing medicated waters, was exhibited, which was corroded completely through its substance, at those points where it had been supported with wood and tied with twine. At these points a black substance was formed, consisting of oxide and chloride of lead. It was obvious that the effect was to be attributed to galvanic action.

24. Dr Barker described a new mode of separating the peroxide of iron by means of acetate of potash. The latter salt, when added to a solution of per-salt of iron, precipitates the peroxide when the liquid is boiled. This would appear to afford an elegant method of separating iron from manganese. He made an observation relative to the precipitation of magnesia by phosphate and carbonate of ammonia; viz. that the same precipitation takes place with bi-carbonate of potash, and other salts.

25. Dr Geoghehan suggested the advantage of employing the double salt of iodide of potassium and bicyanide of mercury, for the purpose of detecting muriatic acid in prussic acid. Sulphuric acid is frequently met with in prussic acid, but the distinction between these two acids is readily made, by means of nitrate of barytes. The peroxide of mercury, usually employed for testing the purity of prussic acid, is ambiguous in its action, as it is usually impure. The use of this salt is not applicable to the alcoholic prussic acid.

26. Mr Johnston made some observations on the iodides of gold, which he had analyzed. Their composition is similar to the chlorides. Previous errors, he found, were to be ascribed to the precipitation of an excess of gold, when ammonia was employed in the analysis. There are three compounds, viz. (1.) $Au\ I$; (2.) $Au\ 3\ I$; (3.) $Au\ 3\ I + K\ I$, the atom of gold being 25.

27. Dr William Barker made some observations on the passage of electricity along a platinum wire. Black spots were observed at regular distances, the rest of the wire being luminous.

28. Mr Scanlan exhibited a beautiful specimen of *hematine* crystallized in the centre of a mass of logwood.

SECTION D.—ZOOLOGY AND BOTANY.

President—Dr ALLMAN.

Vice-Presidents—Dr DAUBENY; Dr GRAHAM.

Secretaries—Mr CURTIS; Dr LITTON.

Committee.—Mr Babington. Dr Coulter. Mr Cristy. Mr Allan Cunningham. Mr D. Don. Mr J. T. Mackay. Mr Maund, Mr W. Nicol. Dr Neill. Mr Parker. Col. Sykes. Mr R. Taylor.

Monday, 10*th August.*—1. A paper was read by Mr Niven on the formation of a natural arrangement of plants for a botanical garden, together with a drawing to illustrate his views.

2. Mr Ball, M.R.E.S., exhibited *Pentacrinus europæus*, and *Beroe ovatus*, in a living state, from Howth. Mr Babington exhibited *Scirpus Savii*, which he found in abundance in Anglesea, near Holyhead, and Dr Graham mentioned that he had observed it on the coast of Galloway. A letter was read from Mr William Hamilton, inclosing remarks on some of the vegetable productions of Mexico, from the pen of Dr Schiede, but which have already appeared in foreign journals. Dr Graham stated that he had lately found in Galloway the *Orchis pyramidalis*, and Dr Knapp that he had met with the same plant near to Edinburgh. Dr Graham also stated, that he had discovered, among the rocks of the coast of Galloway, the *Ononis reclinata*, an annual species, and new to Britain. Mr Babington stated, that M. Reichenbach says there are decidedly three species confounded under *Orchis bifolia*, and that two of them are British, and are readily distinguished by the form of the anthers. The Rev. Dr Drummond informed the members, that, from observations lately made by him, the *Gordius aquaticus* seemed to be viviparous.

Tuesday, 11*th August.*—3. Mr J. T. Mackay, of the College

Botanic Garden, read a short communication from Archdeacon Vignoles, relative to bog timber, from the county of Westmeath, accompanied by specimens from a bog eighteen feet deep. It is remarked in the communication, that there are in several of the bogs three layers of trees, with a stratum of peat between each layer, from three to five feet in depth; that as the trees in the different layers had arrived at maturity, they could not have been coexistent; that in most cases the timber bears marks of fire; and that the roots are rarely attached to the trees. Mr Mackay remarked, that these specimens of bog wood appeared to belong to the *Pinus sylvestris*, but that it is supposed that the *Pinus pinaster* was formerly very abundant in the south of Ireland. Many of the woods are useful in building, and for domestic purposes; and the fir, when split up, is used for candles. Reference was made to the work of Colonel Colby on the subject. Colonel Sykes remarked, that he had observed similar arrangements in the bogs of Scotland; that they contained three distinct layers of trees, the first layer a foot from the surface, quite fresh; then a layer of peat; next a layer of wood, slightly carbonized; under that a layer more carbonized, and slightly bituminised.

4. Colonel Sykes read a paper, entitled "Notices of the Geographical range of certain species of birds common to India, the Cape, and Europe." The common sparrow, stonechat, sandpiper, snipe, golden plover, and coot, were amongst the numerous examples in his collection, and that of the Royal Museum of Edinburgh. In conclusion, he thought it might bear upon the question of the necessity of applying artificial heat in our vivariums, and stated that it suggested the possibility of fossil tigers, elephants, &c. having lived in a temperate climate. Captain James Ross had remarked, that the stonechat, whitethroat, and golden plover, were inhabitants of Hudson's Bay; that the raven occurs also in the arctic regions, and, if it be the same species as that of India, extends over a range of 130°: this, however, has the bill differently formed. The plumage of the raven, he has observed, never varies in the northern regions, although the same individual birds were subject to a variation of between 80° and 90° of temperature.

5. Mr Nicol read a paper on the structure of the horizontal

branches of the natural family of Coniferæ. He stated that in these branches the pith is always nearer the upper than the under side; that the upper side is of a paler colour than the under side; that the upper side is softer and less dense than the under side; and that, whilst the upper side has a structure similar to that of the stems, the under side has a structure so different in all the three principal sections, that without ocular proof, no one could imagine it to belong to the very same branch. The transverse section has the partitions forming the net-work of the under side considerably thicker than those of the upper side. The vessels or openings of the former are consequently smaller than those of the latter, and hence the greater solidity of the wood on the under side. The longitudinal section, parallel to the radial partitions of the under side, have smaller, less numerous, and more obscure discs than those in the upper side. The vessels or spaces containing the discs in the under side present numerous decussating fibres, which do not occur in the upper side; and these fibres also occur in the longitudinal concentric section of the under side. The difference of the structure in the upper and under sides of these branches may be seen very accurately delineated in the third plate of the sixteenth volume of this Journal. The three upper figures are representations of the structure of the upper side, and the three under figures are representations of that of the under side. The branches of ten different species of pines were examined, and the same structure was observed in them all, although in some it is better defined than in others. The peculiarity of the structure in the branches was stated as an additional proof of the absurdity of attempting to constitute new fossil genera, on the supposition that a single slice of a Scotch or spruce fir is characteristic of the whole family of coniferæ. In conclusion, Mr Nicol observed, that an accurate knowledge of the anatomical structure, would sometimes enable the botanist to classify aright when the external characters might leave him in doubt, and in proof of this was observed, that, had the structure been known, the miro of New Zealand would not have been represented as a podocarpus. The structure of that tree bears no resemblance to that of any of the coniferæ, its character being that of a true dicotyledon. It was also mentioned that the Tasmannia dipetala (insipida of Brown), which has been classed with the magnolia, is decidedly not a dicotyledon, it having the structure of the coniferæ; and although the texture is more minute, and less defined than that of any of the larger species of Araucaria, yet it is evidently allied to the Araucarian division. Since the meeting of the Association at Dublin, Mr Nicol has received from Mr Allan Cunningham a bit of the wood (probably a branch) of the Tasmannia aromatica, and he finds, that although there is a peculiarity in its structure, yet there is not a doubt of its resemblance to the dipetala. The peculiarity alluded to, consists of curvilineal rays proceeding from the pith to the surface. These are composed of one or two rows of quadrangular apertures, three or four times larger than those constituting the intermediate spaces.

6. Dr West made a communication on a specimen of Bog Yew, in which the interior portion of the heartwood is completely detached from the exterior.

7. Dr Jacob read a paper on the infra-orbital cavities or larmiers of the French authors, existing in the Deers and Antelopes. This paper is the result of inquiries made by him on the recommendation of the Zoological Committee in 1833. These sinuses are follicles or sacs capable of admitting the end of the finger, existing below the inner canthus of each eye. Dr Jacob shews that they are not receptacles for the tears, as the term *larmier* implies,—the gutter which exists in some from the eye to the cavity, being in many species inadequate for the passage of the tears, and the inspissated residuum found in the sac, not being such as should remain after the evaporation of that liquid. He explains the statements of the Rev. Gilbert White and Major Hamilton Smith, that these sacs communicate with the nostrils through the *puncta lachrymalia*, which these observers suppose to come through sacs or sinuses, which are altogether impervious. In fact, it appears that these follicles are destined for the same purpose for which similar secretions are provided in different parts of other animals, as on the side of the head in elephants, the back of the piccary, the face of certain bats, the belly of the musk, and the numerous præputial and anal glands of others. This is particularly exemplified by the existence of a peculiar black secretion, which exudes in large quantity from

infra-orbital sinuses in the Antilope grimmea, and in the existence of solid masses, like indurated ear wax in old stags.

8. Dr Daubeny read a paper on the circumstances affecting the exhalation of moisture from the leaves of plants. Professor Daubeny reported that, since his communication to the British Association at Cambridge, when he had ascertained that the quantity of carbonic acid decomposed by a plant was in proportion, not to the chemical or heating influence of the ray transmitted to it, but to its illuminating power, he has found that the functions of exhaling moisture by the leaves, and absorbing it by the roots, depend upon the same law: with this difference, however, that, provided some light be present, much heat will serve as a substitute for our transmitting a greater degree of light. He has made experiments which serve to shew, that, so long as the plant continues healthy, in the mutual action of the plant and atmosphere, the balance is always in favour of the purifying influence of plants. Dr Daubeny had employed Drummond's light, but he could not discover that it had any influence on the functions of the plant.

9. A conversation having taken place regarding the occasional torpidity of birds, Dr Neill informed the members, that he knew of a land-rail having been found in a torpid state under a heap of stones and turf in winter; that it was reanimated by heat, but died two days after.

10. Mr James Drummond Marshall read an account of the zoology of the Island of Rathlin, on the coast of Antrim.

11. The President laid before the Section a diagram shewing his plan of a natural arrangement of plants, and made some observations illustrating the principle.

12. A member exhibited a substance called Bog-tallow, found in a globular form, of 6 lb. or 7lb. weight, and which appeared as if it had been inclosed in a tub. It is used in some places to make candles, and, being melted and dropped in water, it assumed the appearance of tallow. Mr Marshall stated that he had found a tub like a firkin, containing a similar substance; but it was the opinion of Dr Neill, that such substances were the adipocere of elks or other animals that had perished there.

13. Dr Marshall observed, that many fishes had disappeared from the northern coast of Antrim; and Dr Neill instanced the

disappearance of the haddock from the Frith of Forth for twelve years, when it again appeared.

14. Mr Sturge of Birmingham, read an account of a toad found alive embodied in a solid mass of new red sandstone; it was stated to have been found at the depth of 10½ feet in a small cavity, and it lived four days. Mr Mackay informed the meeting that he had found several toads at Killarney about thirty years back, and that they are there called black frogs. It was Mr Curtis's opinion, that several species of toads would be discovered in these kingdoms, if they were carefully examined; and Mr Babington alluded to the two species already described.

15. Dr Martin Barry communicated the result of his observations on the colour of the sky, as seen from the summit of Mont Blanc. Dr Barry further noticed, that the effect seemed to be modified by the position of the sun in regard to those portions of snow from which such rays came; and it appeared to him that some discrepancies on the results obtained by Saussure in his Cyanometrical Observations at the Col du Géant and at Chamonix, were referrible to the latter cause.

16. Mr Charles W. Hamilton of the Horticultural Society of Ireland, gave Mr Mackay (who made this communication) an account of a yew found in a bog in Queen's County, in which he was able to count annual rings indicating a growth of 545 years. Yet so compact was the wood, or so close the layers, that the diameter of the trunk did not exceed a foot and a half. The growth had been very slow during the last three centuries, for near the exterior there were about 100 rings within the space of one inch. From the size and number of the yews found in Ireland, and the elevated stations they take amongst the rocks when they assume the stunted appearance of the juniper, Mr Mackay has no doubt of its being a native tree. He exhibited the common and the Florencecourt Yew, a beautiful variety, growing like the cypress. He added, that the seeds of the Irish yew would produce the common tree; but Dr Graham suggested, that as there might be mules, it would not prove that they were the same species. Mr Babington stated, that another variety had been discovered, in which the horizontal branches produced

only drooping sprays on the under side, and that both sexes of the flowers were detected.

17. Mr Mackay read a list of plants peculiar to Ireland, making occasional observations on them.

18. Dr Graham, in allusion to the localities of plants, stated, that some species that were in abundance around Edinburgh, were lost on the west coast of Scotland, and those common again to those districts, he had not seen in Ireland. Mr Curtis remarked upon the *Melampyrum arvense*, that it would only grow amongst corn, and had never been cultivated from seed. He observed, that the plants about Clifton near Bristol, were the same as those found at the back of the Isle of Wight, and that the plants and insects in the south-west of Ireland appeared to be identical, to a great extent, with those of Hampshire and the south of England. He had found the three species of Pinguicula growing in a small space near the Gap of Dunlo, Bartsia viscosa in abundance near Kenmare, and the Eriocaulon septangulare was detected by Mr Haliday and himself in most of the lakes from Oughterard to Roundstone Cunnemara; and Mr Mackay said he had found the *Rhynchospora fusca* at the former station. Mr Fox suggested, that a botanical map, presenting by a variety of colours the localities of plants, would be useful in giving at one view the vegetation of districts.

19. Mr Saunderson called the attention of the meeting to a curious extract he met with in an old Scotch history. It stated, that the northern part of Ireland was so much infested by yew-trees, that a great emigration of Irish took place in consequence, who, with their families and cattle, went over to settle themselves in Scotland, these yew-trees every year destroying their cattle in Ireland. Dr Litton said, he had tried the age of the celebrated yewtree at Muckrass by Decandolle's test, and found that it nearly agreed with the traditional one. He also exhibited a specimen of an oak-tree, bearing the impress of letters on the inner concave surface, which corresponded with the observations made at a previous meeting by Dr West.

20. The Rev. Charles Mayne exhibited specimens of Echini and Asteriæ, that were preserved by being steeped in a solution of chloride of lime, which even prevented the spines of the former from falling off. The preparation should not be so

strong as to act sensibly on the surface of the crust; in that case he had found that the spines would fall off.

Thursday, 13th August—Many of the members being engaged to dine at Salthill, Kingstown, made a botanical and entomological excursion in the forenoon to Killiny Hill; and the meeting of the Section was, therefore, merely formal.

Friday, 14th August.—The Section having met this morning, according to appointment, at the Botanical Garden of the Dublin Society at Glasnevin, the meeting was adjourned to the following day, and Mr Mackay conducted a party on a botanical excursion to Portmarnock and Hill of Howth.

Saturday, 15th August.—Professor Agassiz delivered an extempore discourse on the principles of classification in the animal kingdom in general, and among the mammalia in particular. Although the general groups of animals are impressed with such characters as to be easily recognised, and to admit of little doubt, yet their order and succession has been determined by *no general principle.* This appears from the discrepancy in the positions assigned to them by the most eminent systematists, each of whom has assumed *arbitrarily* some one organ, or system of organs, for the basis of his arrangement. Professor Agassiz, after adverting to some German naturalists who alone have sought after a general principle, and aspired to the character of "philosophic naturalists," passed in review the classes of the animal kingdom, each of which he shewed to exhibit, in an eminent degree, the developement of some one of the animal functions. While vertebrate animals (with man their type) arrive at the greatest perfection in the organs of the senses; the invertebrate offer, in the class of worms, the representative of the system of nutrition; in crustacea, of circulation; in insects, of respiration; and in mollusca, of generation. The Professor next proceeded to demonstrate in what manner the subclasses of vertebrate animals derive each its peculiar character from some one element of the animal economy. This predominant element is the bony skeleton in fishes, the muscular structure in reptiles, the sensibility of the nervous system in birds, and the perfection of the senses in mammalia, which therefore reproduce the distinguishing character, and constitute the type of vertebrate animals. He next shewed that each of the other subclasses of the higher group is represented among the mammalia, along with its own peculiar type. He explained his reason for the four-fold division which he had adopted in this subclass, pointing out the close affinity which connects the Ruminantia, the Pachydermata, the Rodentia, the Edentata, and the herbivorous Marsupialia, in none of which is the true canine tooth developed, and which he considers as forming a single group: while in another he reunites those characterized by the presence of the canine tooth in its proper function (as an instrument of nutrition, not merely of defence), the Carnivora, and those Marsupialia which partake of their character, and the Quadrumana: the Cetacea form a group in themselves, and Man another. The manner in which these represent the subclasses of vertebrata was exhibited by the comparison of cetacea with fishes, ruminantia with reptiles, carnivora with birds, while man is the perfection and type of the mammiferous conformation. Professor Agassiz then applied this principle to illustrate the order and succession of the groups in mammalia, by a reference to the order in which the fossilized remains of the vertebrata occur in the stratified deposits,—1st, Fishes; 2d, Reptiles; 3d, Birds; 4th, Mammalia. From which results the following arrangement of the representative groups among these last: 1st, Cetacea; 2d, Ruminantia, &c.; 3d, Carnivora; 4th, Man,—who thus, in a two-fold aspect, becomes the culminant point of the animal creation.

SECTION E.—MEDICAL SCIENCE.

President.—Dr PRITCHARD.

Vice-President.—A. COLLES, M. D., and PHILLIP CRAMPTON, M. D., Surgeon-General.

Secretaries.—ROBERT HARRISON, M. D., and J. HART, M. D.

Committee.—Mr Adams. Dr M'Dowel. Dr O'Beirne. Mr Carmichael. Dr Houston. Mr White. Dr Osborne. Dr Lendrick. Dr Graves. Dr Stokes. Dr Collins. Dr Alison. Dr Granville. Sir A. Crichton. Professor Jeffray. Dr Traill. Dr Williams. Dr Broughton. Dr Newbigging. Dr Graham. Professor Clarke.

Monday, 10*th August.*—1. The first paper was read by Dr Graves, Professor of the Institutes of Medicine, on the use of chloride of soda in fever. In conclusion, the author read a letter from Dr Stokes, in which he, from experience, stated his high approbation of the remedy.—The second paper was from Dr Houston, Demonstrator of Anatomy, Royal College of Surgeons, Ireland, on certain peculiarities of the circulating organs in diving animals. Though principally carried on and continued by the vital principle, and ceasing altogether when that principle becomes extinct, yet circulation is, to some extent, amenable to the laws of hydraulics. The circulation through a limb is affected by position; the removal or increase of atmospheric pressure is known to affect the circulation in the skin; the action of the air and chest, during respiration, also modify the circulation. It is a well established fact, that a whale can suspend respiration for twenty minutes, and sink to the depth of an English mile in the ocean. The effect of the interrupted respiration, and increased external pressure, must necessarily be an alteration in the circulation, which, unless met by a suitable provision, must prove inconvenient, if not fatal, to the animal. That such a provision existed, Mr Houston was prepared to prove; it existed in the enormous size and complexity of the venous system, particularly in the right cavities of the heart, the venæ cavæ, the hepatic veins, and those of the abdomen and spinal canal. These facts were illustrated by some elegant preparations of the injected venous system of the seal and porpoise. The subject was farther pursued, and the circulation of some animals possessing the power of suspending respiration for a less considerable time was examined; the result was highly satisfactory; and it was proved, by demonstration from several specimens, that the enlargement of the right side of the heart and venous system was proportionate to the power of suspending respiration.

Tuesday, 11*th August*.—2. The first paper was Report of the Dublin Committee on the motions and sounds of the heart, which gave rise to a long discussion.

Wednesday 12*th August*.—3. Dr M'Donnell of Belfast read a paper on the pulse and breathing in early life. After some preliminary observations on the importance of paying strict attention to the posture, in all cases where the pulse is concerned, as it was a well ascertained fact, that the number of pulsations in a given time varied with the posture of the individual on whom the observation was made. Dr M'Donnell stated, that this variation of pulse from posture, which he begged leave to call the differential pulse, was not the same in health and disease, being sometimes greater in diseased conditions, and sometimes altogether absent; and when absent, that disease is invariably present, most probably of the heart or great vessels. One remarkable case was adduced, in which the change from the recumbent to the standing posture produced a doubling of the pulse. The pulse of young children is well known to be very rapid; but that of the fœtus Dr M'Donnell stated to be very slow, which becomes doubled at birth. He considered the condition of the fœtus before birth to be similar to that of cold-blooded animals in its circulation. The objections of stethoscopists to this, he would say, presented a difficulty, but not fatal to his position, as the rapid beating of the fœtal heart, heard through the stethoscope, might proceed from the contractions of the ventricles not being synchronous. That the beating of the heart should be so distinctly heard, though inclosed within the abdomen of the mother, the uterus, membranes, and liquor amnii, would not appear so strange, as it appears at first, from the following experiment: If the faintest-ticking watch be enclosed in an oiled silk bag, and suspended in a vase filled with water, the beating will be heard much more distinctly by the ear applied to the vase than it was whilst the watch was in the open air. Dr M'Donnell adverted to the strength of the pulsation observed, frequently even before birth, in the umbilical cord, and gave an ingenious solution of its cause. In conclusion, he made a few observations on the effect of descending or ascending from the surface of the earth; on the quantity of carbonic acid exhaled from the lungs; and, from considerable variations in that quantity, without any inconvenience being sustained by the individual, he concludes, that the decarbonization of the blood is the least important part of the function of respiration.

4. The next paper was from Professor Harrison on the small bones found in the hearts of certain ruminantia. He commenced with general observations on the circulatory organs, particularly as to their mechanical structure, as suited to the differ-

ent classes of animals, as instanced in the difference of structure observed in the heart, in fishes, reptiles, birds, and mammalia; a singular provision, constantly found in the heart of a division of the latter class, it was the object of this paper to describe, viz. bones found in the heart of the ruminantia. The example and specimens presented to the section, were those of the heart in the common ox. The Professor exhibited and demonstrated the two principal bones, from dried specimens and recent preparations, showing their situation and attachment, by various sections. These bones were found neither in the horse nor stag. They had been mentioned by some writers on Comparative Anatomy as accidental ossifications, found only in the adult animal, particularly in the male. This was an erroneous view; they were constant, and found in the calf as invariably as in the adult, in both male and female. The principal purposes assigned to these productions, were, to preserve the patency of the aorta, which would be endangered from the enormous muscularity of the ventricle in these animals, to serve as a fixed point of action to the muscular fibres, to prevent total closure of the ventricles, and to protect the large sinuses from the powerful resilience of the aorta; the latter purpose was also assigned by Dr Harrison to the very remarkable hard mass of fat found at the base of the heart in these animals.

5. Dr Houston read an interesting memoir on hydatids, and Professor Harrison on the entozoa occasionally found in the voluntary muscles of the human subject.—Dr Roe of Cavan stated a case of a living leech having been discharged from an abscess which he opened on the leg of a boy. The leech must have entered the limb several days previous, and retained its vitality under this confined and novel situation.—The next paper was from Dr Collins, being an abstract of a registry of seven years in the Lying-in Hospital of Dublin.—The next paper was from Sir James Murray, on the influence of the pressure of the atmosphere in certain local diseases; and Professor Jacob, at the same meeting, read some interesting observations on the mammary glands in the cetacea. At 4 o'clock the Section adjourned.

Thursday, 13*th August*.—6. The first paper read was a valuable report on poisons by Dr Roupell.—Dr Alison next read to the section a very important and ingenious memoir on the state of arteries leading to inflamed parts.—Dr Whatton of Manchester explained a new mode of performing partial amputation of the foot, illustrated by casts, preparations, &c. by a living specimen of a successful result of the practice.—Dr Stokes read a series of observations on certain diseases of the chest.—Dr E. Kennedy, on the treatment of virulent ophthalmia in children.—Mr Hart read a successful case of Cæsarean operation, communicated by Mr Knowles, of Manchester.—The next paper was from Dr Perry, on typhus fever.—Afterwards, M. L'Estrange described an improved drill catheter for calculus; and, lastly, Mr Hawkins, described his patent Electriser.

Friday, 14*th August.*—7. A valuable paper from Dr O'Bierne on the functions and diseases of the digestive organs was read. —A paper from Dr Osborne on the effects of cold on the human body was next read to the meeting, and excited much attention.—A paper from Dr Hutton on a congenital dislocation of the hip, and other malformations. This was illustrated by several drawings, and gave rise to a discussion in which Drs Granville, Handyside, and Harrison took part.—Mr Adams read a paper on aneurism by anastomosis; and Dr Handyside presented a paper on the different offices of lacteals, lymphatics, and veins, in the function of absorption.

The Section thereafter adjourned.

SECTION F.—STATISTICS.

President.—Mr BABBAGE.

Vice-Presidents.—Colonel SYKES; Rev. E. STANLEY.

Secretaries.—Mr GREG; Dr LONGFIELD.

Committee.—

Monday, 10*th August.*—1. A paper was read by Dr Mansell, on the statistics of the Dublin Foundling Hospital, upon which some remarks were made by Dr Dickinson, Mr Babbage, Col. Sykes, and several other gentlemen.—A paper was then presented from Dr Cleland of Glasgow, on prison discipline, and copies distributed.—Colonel Sykes presented some documents relating to the new colony of Australia.—He also reported, that his statistical returns having been placed in the hands of Professor Jones, to facilitate the progress of a statistical work, he did not think their withdrawal advantageous to the ends of science, and therefore suspended his digest for the British Association until the completion of Professor Jones's work, after which, should there be matter untouched suitable to the views of the Association, Colonel Sykes proposes to fulfil the recommendation of the section of Statistics.

Tuesday, 11*th August*.—2. Mr Langton, at the request of the Committee, read an abstract of the report of the Manchester Statistical Society, on the state of education in Manchester. The printed report was then presented to the meeting. It contained tables expressing the numbers of schools of different classes, and also the numbers of scholars attending them, the extent of the education afforded them, the mode in which the schools are supported, and the particulars connected with them. A table was also presented, shewing a general summary of the schools and scholars in the burgh of Bury, Lancashire, made in July 1835.—Mr Langton at the same time presented the forms in which the Statistical Society of Manchester had pursued their enquiries into the state of education, into the condition of the labouring classes, and into the means existing for religious instruction. Some observations were then made by Dr Dickinson, who gave the meeting an account of the Normal School in Dublin, established by the Commissioners of Education.—Mr Greg, at the request of the committee, read a paper on the social statistics of the Netherlands, shewing the proportion of crimes of different kinds committed, and the state of education in different districts, and the relation between education and crime, and also the proportion of those accused who are convicted in England, France, and Belgium.—Mr Halsewell made some observations on the causes which affect the proportion between the numbers of those convicted and accused in the metropolitan districts, and on the effects which well-managed houses of correction have in repressing crime. Some additional observations were made by Mr Davenport and several other gentlemen.—Mr Stanley read a report on the Glasgow bridewell, drawn up for the British Association by Dr Cleland of Glasgow. Mr Halsewell and Mr

Stanley made some observations on the facts stated by Dr Cleland, and the meeting of the section was adjourned to 11 o'clock to-morrow.

Wednesday, 12th August.—3. Colonel Sykes, at the request of the meeting, read his paper on wages in the Deccan, accompanied by a table, shewing the gradual improvement in the condition of the labourers in those parts of India which were under the British government, as proved by the decreasing price of necessaries, and the increasing rate of wages. The thanks of the meeting were returned to Colonel Sykes. Professor Babbage made some observations to shew that the high wages of English workmen were caused by their superior skill and industry, and that work was done cheaper in England than in any part of the world.—Mr Babbage read a paper on co-operative shops, for the purpose of supplying workmen with necessaries. He gave the details of an instance in which such shops were in operation from 1818 to 1832. He produced tables, shewing the numbers of purchasers, the quantities of goods sold, and the rates of profit in every year during that period, and explained to the meeting the causes which led to the failure and abandonment of the system in 1832. A conversation ensued, in which the question was discussed, whether the failure was produced by causes essentially inherent in the institution of co-operation systems, or whether they were produced by improper regulations, and could be removed by suitable checks.—Dr Mansel read an abstract, drawn up by Dr Vignolles, of the return made by the coroners on the number of infanticides in the province of Leinster. The returns were confessedly defective, but appeared to shew a slight increase of the prevalence of infanticide since the closing of the Foundling Hospital of Dublin. The meeting was then adjourned until to-morrow.

Thursday, 13th August.—4. Colonel Sykes read a paper on the state of education in the Deccan.—The Rev. E. Stanley read a paper on the religious attendances and state of education in the parish of Alderley, in Chesshire.

5. Dr Reid delivered his views upon a plan tried in Edinburgh for the extension of the study of Physics. In the communication he made to the Section, he confined his attention principally to chemistry. He proposed to have large classes formed for

performing chemical experiments, and that nothing should be employed in these experiments which was not easily procurable by every person. A bit of glass, such as glaziers throw away, a piece of charcoal, and a blow-pipe, would be instruments enough with which to make thousands of experiments, and these would illustrate the essential operations of chemistry. By this means much knowledge would be obtained, and also the mode of conducting an examination on a small scale might be taught with facility. Dr Reid then illustrated the manner in which the experiments were performed in two classes, in one of which 100 mechanics operated at the same time, and in the other 40 young persons. In the first class, the pupils arranged themselves along five ranges of gas-lamps, supported on a narrow board, one being placed alternately on either side. Each was supplied with slips of flat glass, test paper, filtering paper, a test tube, charcoal, and a blowpipe. Materials for each successive experiment were placed at the extremity of each range, and explanations were given during the whole time that the operations were carried on. Until the use of the slips of glass occurred to him, Dr Reid had been unable to introduce, to any extent, the study of practical chemistry in junior schools and mechanics' institutions; but with these, properly selected, the student would effect solutions, evaporate, boil, filter, crystallize, carry on deflagrating experiments, and illustrate practically a wide range of furnace processes, operating on a very small scale, and with a common lamp. Dr Reid then performed a number of experiments with the slips of glass, and stated, that he considered the use of this simple apparatus of great value, not merely in enabling many to become acquainted with chemical science, who might not otherwise have thought of it; but also as affording the means by which the professional student might be enabled to repeat again, and again on a small scale, at home, at the most trifling expense, the greater number of the experiments which he sees while attending an extended course of lectures, or which he may conduct on the larger scale in the practical course given to professional chemists. From calculations made in different places, he found that from L.2 to L.5 would provide apparatus and materials sufficient to show many thousand experiments.

The great object was to render this department of knowledge accessible to all persons, and particularly to young persons.*

Friday, 14*th August*.—6. Mr Babbage read an abstract of the ordnance survey of the city of Londonderry and parish of Templemore.—It was then resolved that the Statistical Section of the British Association should present their thanks to the Lord Lieutenant of Ireland, for the presentation to that Section of a copy of the Survey of the City of Londonderry and parish of Templemore; and in stating their admiration of the extensive and valuable statistical details contained in it, they beg to express their hope, that the general survey may be completed in the same spirit in which it has been commenced.—Dr Jones of Waterford read an essay on the statistics of lunacy.—Two papers read, which were presented by Mr Fox, on the punishment of death in Russia, Norway, Belgium, and Brunswick, and the Section adjourned.

EVENING MEETINGS IN THE ROTUNDO, &c.

Monday.—A general meeting of the Association was held in the Rotundo on Monday evening. More then 2000 persons were present. The meeting was opened by the President of the former year, General Sir Thomas Macdougall Brisbane, who addressed this splendid and interesting assemblage in the following terms:—

MY LORDS, LADIES, AND GENTLEMEN,—We are now assembled in the capital of Ireland, for the purpose of holding the Fifth Anniversary Meeting of the British Association for the Advancement of Science. We all must admire the excellent arrangements which have been made for our reception, and the unbounded liberality by which they are characterized. It is impossible to anticipate the vast advantage likely to result to science from such meetings, where there is such a mighty combination of talent, as is to be found embodied amongst the members of the Association, which may be said comprehends every department of human knowledge, whose pure and only object is the advancement of science, and the improvement of the condition of man. When you did me the honour to elect me to the distinguished office of President, I accepted it with diffidence. I have used my best, though humble, exertions to fill the situation with propriety, and I shall resign it with much satisfaction into the far abler hands of your highly gifted and enlightened countryman, the

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* Within the last few months, a number of schoolmasters in London, Edinburgh, and Dublin, have stated their intention of introducing the plan proposed by Dr Reid, and some have already announced it in their advertisements.—B. R.

Rev. Dr Lloyd, of Trinity College. Although I expressed my diffidence in accepting the high office of president, you are not to infer that I was indifferent to the distinguished honour bestowed on me in that appointment. On the contrary, I look upon it as the highest I ever enjoyed. The celebrated Dutch geometrician, Van Keulan, who devoted the greater part of his life to the task of ascertaining the exact proportion between the diameter of a circle and its circumference, and, in order to arrive at the utmost degree of accuracy, carried it to one hundred places of figures, directed at his death that these figures should be engraved on his tombstone. In imitation of Van Keulan, it shall be my desire that the circumstance of my having been President of the British Association be recorded on my grave, as the most exalted tribute to my memory. I need not infringe on your valuable time by dwelling on the wide field for scientific inquiry which Ireland presents, or enumerating the many illustrious individuals to whom she has given birth. They are familiar to us all. Neither need I tell you, that her literature has a venerable claim to antiquity. All Europe acknowledges the talents of a Burke, a Sheridan, a Swift, a Goldsmith, a Kirwan, an Usher, and many others who are gone to their rest. In our days, we look up to these bright constellations, by some of whom we are surrounded—a Brinkley, a Robinson, a Hamilton, and, though last, not least of her illustrious sons, the immortal hero of Waterloo. No man who has resided in Ireland, as I have done for nearly four years, can have left that country but with a deep impression, arising from the uniform kindness, hospitality, and friendship, which every stranger receives from her warm-hearted sons; and it is with the utmost sincerity I assure you, that the days I have passed in Ireland have been among the happiest of my life. Few individuals have had more extensive opportunities of witnessing the gallantry, good conduct, and cheerful subordination of the Irish soldier, under all the various and trying circumstances incident to the service; and I can bear ample testimony to the fact, that no men have deserved better of their country. I fear you will consider me as having travelled out of my record in touching upon military matters; but the partiality which I entertain for my profession must plead my apology. We have much to regret the absence of many of our enlightened members. At the head of the list I should place the Bishop of Cloyne; and what embitters the consideration of his absence is, the reflection that it arises from illness. As I perceive we have been attacked in some of the periodical journals for making too long speeches at our meetings, if I should address you at greater length, I fear I may render myself liable to become the subject of their sarcasm. I shall therefore conclude by moving, that the Rev. Dr Lloyd, who is eminently known to you all, shall take the chair as President.

Dr Lloyd having taken the Chair (on his right hand the Viceroy of Ireland, Lord Mulgrave, and on the left Sir Thomas M. Brisbane), addressed the audience in a speech, of which the following is a summary :—

My Lords and Gentlemen of the British Association,—In offering my acknowledgments for the honour you have done me by voting me into the chair at the meetings of the present year, I persuade myself that I shall not seem to set the less value either on your favour, or on the high distinction it confers, if I express my conviction, that I must owe this my elevation to a circumstance which has long been deplored by the friends of science and humanity throughout this kingdom—I mean to the long continued illness of a truly reverend Prelate, to whom we had been accustomed to ook up to as an eminent leader in the most arduous walks of science;

and in those high walks ever to be seen among the foremost in the march of scientific discovery. There are no claims to your notice which could have been advanced in my behalf, even by my most partial friends, that could have directed you to the choice you have made, but for the unhappy circumstance now alluded to. Those friends (I fondly hope) will feel that they and the University have been complimented in the person of their aged academic superior. For myself, I can truly aver, that the honour you have done me is one to which I never could have presumed to aspire; and that now it leaves me nothing further to wish for, except one thing, and one only, which is, that I were more deserving of so high a compliment. Of the Association itself, its objects, and its construction, whereby it is fitted for the attainment of those objects, it must be unnecessary for me to offer any explanation in this assembly, wherein all this is so well understood; but it may not be so generally known, because it can scarcely be believed, that without these walls there are to be found individuals, though I hope not many, who regard your exertions with something like painful apprehension; finding themselves unable to reconcile the discoveries which have been made in a certain department of science, to which your attention is here invited, to their views of the Mosaic history. With these apprehensions it would be my wish to deal tenderly, if I could but learn how to respect them. I meant not to insinuate that such persons could propose to restrict the investigation of truth in any of the paths which may be supposed to lead to its possession; or that they could possibly think that we should suppress any of the discoveries which have been made, however alarming in their view of them; for this would be (to use the language of Bacon) "*Deo per mendacium gratificari.*" But I do not mean to assert, and I do it most confidently, that they are themselves to blame for that indigestion of which they complain. Happily, however, as their ailment has its source altogether within themselves, so also is the remedy within their own power; and if they would condescend to permit me to advise, I think I could help them with a prescription suited to their case. I would recommend that they should proceed with more patient circumspection, or at least more of self-distrust and of doubting humility, both in their interpretation of the language of the sacred historian, and in the inferences which they venture to draw from certain discoveries which have been made in geological science. It may be perceived that I suspect them of precipitancy in drawing conclusions from views hastily adopted; and as they may not be prepared to plead guilty to this charge, I would beg leave to ask them, have they indeed ascertained how far back the sacred historian had proposed to carry his readers in the communications he was commissioned to make. The answer, perhaps, will be "to the origin of all things to the creation of the universe. It is true that he prefaces his history of God's government over his chosen people, by informing us that "in the beginning God created the heavens and the earth." And it seems equally certain that he here speaks of the original creation of all things out of nothing. This, indeed, is a great subject; and though nothing circumstantial is here revealed to us concerning it, yet the sacred importance of the truth, assured to us by this single expression, is every way suited to the prominent place assigned to it; for it is nothing less than the authoritative statement of the first and fundamental article of all true religious faith. By it we are taught that self-existence is an attribute of the one Supreme Being, and that all things whatsoever beside owe their existence to his supreme power. How necessary it was to mankind to have an authoritative declaration on this subject, we may readily convince ourselves, by adverting to the errors into which the most celebrated men of all antiquity had fallen, who presumed to speculate on these matters, so far beyond the reach of human reason, without other guidance. Among these erroneous and wild opinions we find the following:—That matter was eternal; the Deity was the soul of the world; and, consequently, the material frame of nature was to be regarded as his body, and not as his work, with many other equally presumptuous. Now in this, his first sentence, the inspired writer settles definitively what we are to believe on the subject, by stating the primary relation which all things in common bear to the Supreme Being; and with this information he forbears mixing up any other matter. For it will be perceived, that the statement is made without any specification of time or other circumstance; seemingly because no addition of this kind could be of use in aiding our conceptions of a truth purely religious, or in strengthening our faith in the authority on which it was founded, but chiefly because it was the sole object of the writer, in this first sentence, to claim for God the creation of all things whatsoever. And that this claim must remain unshaken, however we may decide on other questions which may be raised about the creation—such as the question relative to the time when it occurred—how long before the origin of the human race—whether all parts of the universe were brought into existence simultaneous, or at different and widely distant epochs. It is plain, then, that in this place the sacred writer furnishes no help for the decision of such questions. Let us look to what follows. In proceeding to those arrangements by which the earth was to be fitted for the residence and support of man, and the other inferior tribes by which it was then to be tenanted, we find him describing its preceding condition, informing us that it was then "without form and void," and that "darkness was upon the face of the deep." Now I confess that this always seemed to me very like the description of a ruined world; and if such was the earth at that time, it would be difficult to suppose that it had not existed long before. But this is not all. When he does come to the work of the six days, we find the description of each day's work introduced by an expression of a particular form, and concluded by another; by which it appears, that the original work of creation, spoken of in the first verse, is excluded from the series of performances belonging to those days, and if excluded, then perhaps removed to an indefinite distance, for had it immediately proceeded, we might naturally expect to find it spoken of, either as the work of the first of a series of seven days, or as part of the work of the first of the six days. This, then, would seem to remove the work of original creation far beyond that of the reconstruction of the globe. It is true that nothing is exhibited to our imaginations to mark the intervals between these performances, but to deny that there was such an interval, and for that reason, would be to conclude about as wisely as the peasant, who supposes the clouds to be contiguous to the stars, because when looking up he discerns nothing between them. How, then, stands the case between Moses and the men of science? From Moses it is collected by the most learned chronologers that the human race has existed about seven thousand years. According to geologists, the race of man is coeval with the earth in its present form; and judging by the marks of age observable on the features of this latter, they are led to the same conclusion. Again the geologists see reason to believe that the globe, though in some different condition, is far more ancient than the races by which it is now inhabited; and the indications discoverable in the sacred records, as far as they help to decide, are, we perceive, in favour of this notion of its higher antiquity. But some persons may think that the arguments here derived from the sacred records themselves are too weak to establish a coincidence so extensive. Be it so; we are under no necessity of pressing them to this extent. It is enough for us to shew, on the part of men of science, that they offer no contradiction whatever to the sacred historian; and on the part of the latter, that we have this free permission to explore the wonders of creation, for ascertaining whatever they may serve to ascertain; whether it be the antiquity of the globe, the various changes it may have undergone, or the different purposes for which it may have been made to serve, before that it was fitted for, and delivered over to, its present inhabitants. All we ask for Moses is, that he should not be required to exceed his commission in what he records for our instruction, or be interrogated on subjects altogether foreign from those of which it was his purpose to treat. The same precipitancy is often to be observed in the manner in which some men reason on those discoveries which the gracious Author of our being has enabled us to make for ourselves among the work of his hands; and the same check is to be applied. Thus, admitting that geologists have discovered satisfactory proofs of a fixed order of succession in many geological formations, and that they are enabled to judge with tolerable accuracy as to the time they would require for their accumulation by the forces now in operation, whether Neptunian or Volcanian—is it fair from this to conclude at once as to the time actually required—*i. e.* as to the absolute age of these formations, or to ascribe to them an antiquity far greater than that of the human race? They may be of this antiquity, but it is not so established. For is it not reasonable to suppose that the forces concerned, though they should be the same in kind with those now in operation, must in the beginning have acted with indefinitely greater energy than now, that the appetencies and the principles themselves have become comparatively inert by saturation, and consequently the forces with which they are induced, reduced as it were to a condition of equilibrium; or shall we expect to find the same activity in a neutral salt as in its separate elements before chemical combination, or the same tendency to motion in a magnet, whether in or out of the magnetic meridian? Who can doubt that effects may have been formerly produced on the most gigantic scale, in far less time than it would take to effect the most insignificant changes by the same forces, now become comparatively inert? It is not then because of the insufficiency of the period included within the Mosaic history that any can feel themselves compelled to place certain formations, such as that of coal, and many others, beyond the limits of that history; but because of the imbedded remains of plants or animals altogether different from any now in existence, or now existing in the same regions of the earth; or because of the number and variety of the overlying formations, indicative of successive epochs of destruction; or because of such an occurrence, on a large scale, Moses takes no notice, except in the matter of the general deluge; and of the extensive, I would rather say, of the universal, dominion of the waters over the surface of the globe, there remain to us abundant monuments, independently of those brought to light by that ornament of this Association, Dr Buckland, and others. What I insist on, then, is this; that when we seek God through the indications of his power or his will contained in his word or in his works, we should apply ourselves to the task with patient self-distrust and humble reverence, amounting to religious awe. This is the frame of mind which becomes us when we would approach the Father of Lights; and I would add, that this is the frame of mind which every advance in the study of his works, no less than of his word, is fitted to produce. In fact, it is only the grossly ignorant who is insensible to his own ignorance. The more extensive his knowledge, the greater the number and variety of the subjects which present themselves for further inquiry. The wider the sphere of illumination, the more expanded is the surface which separates it from the domain, the more numerous the points in that boundary by which we are sensibly confined. This growing sense of our insufficiency, adequately to comprehend the workings of Divine power, serves but to increase the wonder excited by what is already brought within the compass of our discernment; and whilst man is humbled, God is exalted. Can we then fail to acknowledge, with the illustrious Bacon, the religious uses of natural science, when, in that glowing language so peculiar to himself, he thus expresses his conviction? "Philosophia naturalis, post verbum Dei, certissima superstitionis medicina est; eademque probatissimum Fidei alimentum, itaq.; merito religioni donatur tanquam fidissima ancilla, cum altera voluntatem Dei, altera potestatem manifestat." Perhaps nothing can be more just than this representation of the benefits to religion to be derived from the study of nature; yet I confess, that I have been still more deeply impressed, and that, too, by a young gentleman of fashion, who, with myself, some thirty years ago, happened to join a party on a visit to the splendid gardens of the Dublin Society. On that occasion, a remark was made by one of the company relative to the frailty of the objects which engaged our attention, which was mistaken by him for disparagement. I was glad of the mistake, as it drew from him an expression which brought the truth home to my mind with so much power that I never can forget it. He replied, that he was affected by what he saw in a manner widely different, for that to him "it seemed that the earth we inhabit, with all its magnificent furniture, no less wonderful in its structure than splendid in appearance, was not made for human beings, but for gods." I cannot suppose, that in this vast assembly there is an individual who has not felt the truth, as well as the force, of this sentiment thus expressed; or one who does not perceive what great support it is fitted to afford to the Christian faith. For it will be observed, that the opinions which secretly lie at the root of all infidelity are these—that man is not so bad, nor God so good, as the gospel represents them. Now the latter of these opinions cannot for a moment withstand the force of the observation here alluded to, for it must naturally occur to every mind capable of the least reflection, that if such is the lavish bounty of the Creator to his creatures in this their present state of alienation and hostility, there can be no *a priori* reason whatever for suspecting that the promises of the gospel, however magnificent, are delusive, which have been made to the same objects of his bounty, after that by the Divine teaching they shall be fitted for their true enjoyment. Now, I know it may be said, that all this may be very true, as well as much more that may be stated relative to the great advantages which flow from the cultivation of natural science in each of its numerous departments; but how does it prove the usefulness of this and the like associations, when, after all, that which is to be learned or effected is the work of individuals, to be performed by them when separated from each other, often in their private laboratories, or in their closets; and where, then, the necessity for calling on men to join these meetings, frequently, as it must be, from enormous distances, and great personal inconvenience? This question I would beg leave to answer by another. How account for the fact confirmed by all experience, that those who have secured for themselves immortal honour, by their successful labours for advancing the boundaries of science, have always appeared in groups; distinguishing the ages in which they lived from those which preceded or followed, by their extraordinary brightness, like the luminous bands in some of your optical experiments? Truly the wave hypothesis does not furnish a more satisfactory explanation of this latter phenomenon, than the influence of example, and its force to awaken the dormant powers of genius, does of the other; and this is the influence which we propose to strengthen, by assembling together men who are engaged in the same or kindred pursuits. We know that a burning coal, when taken from among other combustibles, and abandoned to itself, slumbers, and perhaps becomes extinct; but when brought into contact with another combustible, they burn bright and strong by mutual influence, shedding light and heat on all around them. This I take to be an exact image of the effect of the communications and intercourse we have here established. How many hints are here thrown out during your discussions, which, when followed up, lead to the most important results? How many useful suggestions are here offered as to the mode of prosecuting particular discoveries, and how many are roused to exertion by the intercommunication of kindred minds, and supported in their exertions, by knowing that there are others who take an interest in their success? But if it were possible for a moment to overlook these great advantages, and to value these meetings merely as affording the opportunities of rational recreation, I would still appeal to the lovers of science, and the admirers of those by whom it is extended, and I would ask them, where, or in what society, can they hope to find the means of gratification so high, so ennobling, so heartfelt, as that which they may here expect to enjoy? For myself, I can truly assert, that I know of nothing that could bribe me to withdraw my unworthy self from the rolls of this Association, or to deny myself the opportunities here so freely afforded, of cultivating the personal friendships of those who have been heretofore the objects of my distant admiration; and that if this Association had nothing else to recommend it, but that it is the source of such high satisfaction, I should regard its declension as a calamity to be deplored; but this I say without any apprehension of such a result. The danger, if it had ever existed, has already passed. This Association has already exhibited too many and solid proofs of its usefulness, ever to be slighted; it has left too many pleasing recollections ever to be abandoned.

Professor Hamilton then read the following Report:—

It has fallen to my lot, gentlemen, as one of your Secretaries for the year, to address you on the present occasion. The duty would indeed have been much better discharged, had it been undertaken by my brother secretary; but so many other duties of our secretaryship had been performed almost entirely by him, that I could not refuse to attempt the execution of this particular office, though conscious of its difficulty and its importance. For if we may regard it as a thing established now by precedent and custom that an annual address should be delivered, it is not therefore yet, and I trust that it will never be, an office of mere cold routine, a filling up of a vacant hour, on the ground that the hour must be some way or other got rid of. You have not left your homes—you have not adjourned from your several and special businesses—you have not gathered here to have your time thus frittered away, in an idle and unmeaning ceremonial. There ought to be, and there is, a reason that some such thing should be done; that from year to year, at every successive re-assembling, an officer of your body should lay before you such an address; and in remembering what this reason is, we shall be reminded also of the spirit in which the duty should be performed. The reason is, the fitness, and almost the necessity of providing, so far as an address can provide, for the permanence and progression of the body, by informing the new members, and reminding the old, of the objects and nature of the Association, or by giving utterance to at least a few of those reflections which at such a season present themselves, respecting its progress and its prospects: and it is a valid reason, and deserves to be acted upon now, however little may have been left untried in the addresses of my predecessors in this office. For if even amongst the members who have attended former meetings, and have heard those eloquent addresses delivered by former secretaries, it is possible that some may have been so dazzled by the splendour of the spectacle, and so rapt away by the enthusiasm of the time, as to have given but little thought to the purport and the use, the meaning and the function of the whole; much more may it be presumed that of the several hundred persons who have lately joined themselves as new members to this mighty body, there are some, and even many, who have reflected little as yet upon its characteristic and essential properties, and who have but little knowledge of what it has been, and what it is, and what it may be expected to become. First, then, the object of the Association is contained in its title; it is the advancement of science. Our object is not literature, though we have made literary associates, and though we hail and love as brethren those who are engaged in expressly literary pursuits, and who are either themselves the living ornaments of our land's language, or else make known to us the literary treasures of other languages, and lands, and times. Our object is not religion in any special sense, though respect for religious things and religious men has always marked these meetings, and though we are all bound together by that great tie of brotherhood, which unites the whole human family as children of one father, who is in heaven;—still less is our object politics, though we are not mere citizens of the world, but are essentially a British Association of fellow-subjects and of fellow-countrymen, who give, however, glad and cordial welcome to those our visitors who come to us from foreign countries, and thankfully accept their aid to accomplish our common purpose—that common purpose, that object for which Englishmen, and Scotchmen, and Irishmen have banded themselves together in this colossal Association, to which the eyes of the whole world have not disdained to turn, and to see which, and to raise it higher still, illustrious men from foreign lands have come, is Science: the acceleration of scientific discoveries, and the diffusion of scientific influences. And if it be inquired, how is this aim to be accomplished, and through what means, and by what instruments and process we as a body hope to forward science, the answer briefly is, that this great thing is to be done by us through the agency of the social spirit, and through the means, and instruments, and process which are contained in the operation of that spirit.—We meet, we speak, we feel *together now*, that we may *afterwards* the better think, and act, and feel *alone*. The excitement with which this air is filled, will not pass at once away; the influences that are now among us, will not (we trust) be transient, but abiding; those influences will be with us long, let us hope that they will never leave us; they will cheer, they will animate us still, when this brilliant week is over; they will go with us to our separate abodes, will attend us on our separate journeys; and whether the mathematician's study, or the astronomer's observatory, or the chemist's laboratory, or some rich distant meadow unexplored as yet by botanist, or some untrodden mountain top, or any of the other haunts and homes and oracular places of science, be our allotted place of labour till we meet together again, I am persuaded that those influences will operate upon us all, that we shall all remember this our present meeting, and look forward with joyful expectation to our next reassembling, and by the recollection, and by the hope, be stimulated and supported. It is true, that it is the individual man who thinks and who discovers, not any aggregate or mass of men. Each mathematician for himself, and not any one for any other, nor even all for one, must tread that more than royal road which leads to the palace and sanctuary of mathematical truth. Each for himself, in his own personal being, must awaken and call forth to mental view the original intuitions of time and space; must meditate himself on those eternal forms, and follow for himself that linked chain of thought, which leads, from principles inherent in the child and in the peasant, from the simplest notions and marks of temporal and local site, from the questions when and where, to results so varied, so remote, and seemingly so inaccessible, that the mathematical intellect of full grown and fully cultivated man cannot reach and pass them without wonder, and something of awe. Astronomers, again, if they would be more than mere artizans, must be more or less mathematicians, and must separately study the mathematical grounds of their science; and although in this, as in every other physical science, in every science which rests partly on the observation of nature, and not solely on the mind of man, a faith in testimony is required, that the human race may not be stationary, and that the accumulated treasures of one man or of one generation of men may not be lost to another, yet even here, too, the individual must act, and must stamp on his own mental possessions the impress of his own individuality. The humblest student of astronomy, or of any other physical science, if he is to profit at all by his study, must in some degree go over for himself, in his own mind, if not in part with the aid of his own observation and experiment, that process of induction which leads from familiar facts to obvious laws, then to the observation of facts more remote, and to the discovery of laws of higher order. And if even this *study* be a personal act, much more must that *discovery* have been individual. Individual energy, individual patience, individual genius, have all been needed, to tear fold after fold away, which hung before the shrine of nature; to penetrate, gloom after gloom, into those Delphic depths, and force the reluctant Sibyl to utter her oracular responses. Or if we look from nature up to nature's God, we may remember that it is written—" Great are the works of the Lord, sought out of all those who have pleasure therein." But recognising in the fullest manner the necessity for private exertion, and the ultimate connexion of every human act and human thought with the personal being of man, we must never forget that the social feelings make up a large and powerful part of that complex and multiform being. The affections act upon the intellect, the heart upon the head. In the very silence and solitude of its meditations, still genius is essentially sympathetic; is sensitive to influences from without, and fain would spread itself abroad, and embrace the whole circle of humanity, with the strength of a world-grasping love. For fame, it has been truly said, is love disguised. The desire of fame is a form of the yearning after love; and the admiration which rewards that desire, is a glorious form of that familiar and every-day love which joins us in common life to the friends whom we esteem. And if we can imagine a desire of excellence for its own sake, and can so raise ourselves *above* (well if we do not in the effort sink ourselves *below*) the common level of humanity, as to account the aspiration after fame only " the last infirmity of noble minds," it will still be true that, in the greatest number of cases, and of the highest quality,

> Fame is the spur that the clear spirit doth raise,
> To scorn delights, and live laborious days.

That mysterious joy—incomprehensible if man were wholly mortal—which accompanies the hope of influencing unborn generations; that rapture, solemn and sublime, with which a human mind, possessing or possessed of some great truth, sees in prophetic vision that truth acknowledged by mankind, and itself long ages afterward remembered and associated therewith, as its interpreter and minister, and sharing in the offering duly paid of honour and of love, till it becomes a power upon the earth, and fills the world with felt or hidden influence: that joy, which thrills most deeply the minds the most contemptuous of mere ephemeral reputation, and men who care the least for common marks of popular applause or outward dignity—does it not show, by the revival in another form, of an instinct seemingly extinguished, how deeply man desires, in intellectual things themselves, the sympathy of man? If, then, the *ascetics* of science—if those who seem to shut themselves up in their own separate cells, and to disdain or to deny themselves the ordinary commerce of humanity—are found, after all, to be thus influenced by the *social spirit,*—we can have little hesitation in pronouncing that to the operation of this spirit must largely be ascribed the labours of ordinary minds; of those who do not even affect or seem to shun the commerce of their kind; who accept gladly, and with acknowledged joy, all present and outward marks of admiration or of sympathy, and who are willing, and confess themselves to be so, to do much for immediate reward, or speedy though perishing reputation. Look where we will, from the highest and most solitary sage who ever desired "the propagation of his own memory," and committed his lonely labours to the world, in full assurance that an age would come when that memory should not willingly be left to die, down to the humblest labourer who was ever content to co-operate outwardly and subordinately with others, and hoped for nothing more than the present and visible recompense, we still perceive the operation of that social spirit, that deep instinctive yearning after sympathy, to use the power and (if it may be done) to guide the influences of which, this British Association was formed. Thus much I thought that I might properly premise, on the social spirit in general, and its influence upon the intellect of man; since that is the very bond, the great and ultimate reason, of this and of all other similar associations and companies of studious men. But you may well expect that in the short remaining time which your leisure this evening can spare, I should speak more especially, and more definitely, of this British Association in particular. And here it may be right to adopt in part a more technical style, and to enter more minutely into detail, than I could yet persuade myself to do, till I had eased myself in some degree of those overflowing emotions, which on such an occasion as this could hardly be altogether suppressed. Presuming, therefore, that some one now demands, how this Association differs from its fellows, and what peculiar means it has of awakening and directing to scientific purposes the power of the social spirit; or why, when there were so many old and new societies for science, it was thought necessary or expedient to call this society also into being: I proceed to speak of some of the characteristic and essential circumstances of this British Association, which contain the answer to that reasonable demand. First, then, it differs in its magnitude and universality from all lesser and more local societies. So evidently true is this, that you might justly blame me, if I were to occupy your time by attempting any formal proof of it. What other societies do upon a small scale, this does upon a large; what others do for London, or Edinburgh, or Dublin, this does for the whole triple realm of England, Scotland, and Ireland; its gigantic arms stretch even to America and India, insomuch that it is commensurate with the magnitude and the majesty of the British empire, on which the sun never sets; and that we hail with pleasure, but without surprise, the enrolment of him among our members, who represents the sovereign here, and is to us the visible image of the head of that vast empire; and the joy with which we welcome to our assemblies and to our hospitality, those eminent strangers who have come to us from foreign lands, rises almost above the sphere of private friendship, and partakes of the dignity of a compact between all the nations of the earth. Forgive me that I have not yet been able to speak calmly in such a presence, and on such a theme. But it is not merely in its magnitude and universality, and consequently higher power of stimulating intellect through sympathy, that this Association differs from others. It differs also from them in its constitution and details; in the migratory character of its meetings, which visit, for a week each year, place after place in succession, so as to indulge and stimulate all without wearying or burdening any; in encouraging oral discussion throughout its several separate sections, as the principal medium of making known among its members the opinions, views, and discoveries of each other; in calling upon eminent men to prepare reports upon the existing state of knowledge in the principal departments of science; and in publishing only abstracts or notices of all those other contributions which it has not as a body called for; in short, in attempting to induce men of science to work more together than they do elsewhere, to establish a system of more strict co-operation between the labourers in one common field, and thus to effect more fully than other societies can do, the combination of intellectual exertions. In other societies, the constitution and practice are such, that the labours of the several members are comparatively unconnected, and few attempts are systematically made to combine and harmonize them together; so that if we except that general and useful action of the social spirit upon the intellect of which I have already spoken, and the occasional incitement to specific research, by the previous proposal of prizes, there remains little beyond the publication of Transactions, whereby they seek as bodies to co-operate in the work of science. In them an author, of his own accord, hands in a paper; the title and subject are announced; it is referred to a committee for examination, and if it be approved of, it is published at the expense of the society. This is a very great and real good, because the most valuable papers are seldom the most attractive to common purchasers, and because the authors of those papers are rarely able to defray from their own funds the cost of an expensive publication. There is no doubt that if it had not been for this resource, many essays of the greatest value must have been altogether suppressed, for want of pecuniary means. Besides, the approbation of a body of scientific men, which is at least partially implied in their undertaking to publish a paper, however limited and guarded it may be by their disclaimer of corporate responsibility, cannot fail to be accounted a high and honourable reward; and one of which the hope must much assist to cheer and support the author in his toils, by virtue of the principle of sympathy. It is known, and, I believe, was mentioned in an address to this Association at one of the former meetings, that the Principia and Optics of Newton were published at the request of the Royal Society of London. Newton, indeed, might well have thought that those works did not need that sanction if the meekness of his high faculties had permitted him to judge of himself as all other men have judged of him: but our gratitude is not therefore due the less to the society whose request prevailed over his own modest reluctance, and procured those treasures for that and for every age. It must be added, that the Royal and Astronomical Societies print abstracts of their communications, for speedy circulation among their members, which is a useful addition to the service done in publishing the papers themselves, and is an example well worthy of being followed by all similar institutions; and that the Royal Society has even gone so far as to procure and print, in at least one recent instance (I mean in the case of a paper of Mr Lubbock's), and perhaps also in some other instances, a report from some of its members, on a memoir presented by another, thus imitating an excellent practice of the Institute of France, which has probably contributed much to the high state of science in that country. This last procedure, and doubtless other acts of some other scientific societies, such as the discussions in the Geological Society, the lending of instruments by the Astronomical Society to its members, and the occasional exhibition of models and experiments by members to the body, in the Irish and other institutions, are examples of direct co-operation; and, perhaps, there is nothing to prevent such cases being greatly multiplied hereafter. But admitting freely these and other claims of the several societies and academies of the empire to our gratitude for their services to science, and accounting it a very valuable privilege to belong, as most of us do, to one or other of those bodies, and acknowledging that there is much work to be done which can only be done by them, we must still turn to this Association, as the body which is *co-operative* by eminence. The *discussions* in its sections are more animated, comprehensive, and instructive, and make minds, which before were strangers, more intimately acquainted with each other, than can be supposed to be the case in any less general body; the *general meetings* bring together the cultivators of all different departments of science; and even the less formal *conversations,* which take place in its halls of assembly during every pause of business, are themselves the working together of mind with mind, and not only excite, but *are* co-operation.—Express requests also are systematically made to individuals and bodies of men, to co-operate in the execution of particular tasks in science, and these requests have often been complied with. But more perhaps than all the rest, the reports which it has called forth on the existing state of the several branches of knowledge are astonishing examples of industry and zeal exerted in the spirit, and for the purpose of co-operation. No other society, I believe, has yet ventured to call on any of its members for any such report, and, indeed, it would be a difficult, perhaps an invidious thing, for any one of the other societies or academies so to do. For such a report should contain a large and comprehensive view of the treasures of all the academies; and would it not be difficult for a zealous member of any one of them, undertaking the task at the request of his own body, to form and to express that view with all the impartiality requisite? Would there not be some danger of a bias in some things to palliate the defects of his own particular society, and in other things to exalt, beyond what was strictly just, its true and genuine merits? But a body like the British Association, which receives, indeed, all communications, but publishes (except by abstract) none, save only those very reports which it had previously and specially called for;—a body such as this, and governed by such regulations, may hope that, standing in one common relation to all the existing academies, and not belonging to the same great class of societies publishing papers, the members whom it has selected for the task may come before it to report what has resulted from the labours of all those different societies, without any excessive depression or any undue exultation, and in a more unbiassed mood of mind than would be possible under other circumstances. Accordingly, the reports already presented by those eminent men who were selected for the office (and rightly so selected, because a comprehensive mind was not less needed than industry), appear to have been drawn up with as much impartiality as diligence; they comprise a very extensive and perfect view of the existing state of science in most of its great departments: and if in any case they do not quite bring down the history of science to the present day (as certainly they go near to do), they furnish some of the best and most authentic materials to the future writer of such history. But we should not only underrate the value of those reports, but even quite mistake the character of that value, if we were to refer it at all to its connexion with distant researches, and some unborn generation. They will, indeed, assist the future historian of science; but it was not solely, nor even chiefly, for that purpose they were designed, nor is it solely or chiefly that purpose which they will answer. They belong to our own age; they are the property of ourselves as well as of our children. To stimulate the living, not less than to leave a record to the unborn, was hoped for, and will be attained, through those novel and important productions. In holding up to us a view of the existing state of science, and of all that has been done already, they show us that much is still to be done, and they rouse our zeal to do it. Can any person look unmoved on the tablet which they present of the brilliant discoveries of this century, in any one of the regions of science? Can he see how much much has been achieved, what large and orderly structures have been in part already built up, and are still in process of building, without feeling himself excited to give his own aid also in the work, and to be enrolled among the architects, or at least among the workmen? Or can any one have his attention guided to them by any wants that remain,—can he look on the gaps which are still unfilled, even in the most rich and costly of those edifices (like the unfinished window that we read of, in the palace of eastern story), without longing to see those wants supplied, that palace raised to a still more complete perfection—without burning to draw forth all his own treasures of thought, and to elaborate them all into one new and precious offering? The volume containing the reports which were presented at the last meeting of the Association has been published so very recently, that it is, perhaps, scarcely yet in the hands of more than a few of the members; some notice of its contents may therefore be expected from me now, though the notice which I can give must of necessity be brief and inadequate.

[Professor Hamilton then gave a general review of the character and merit of several Reports, but we do not think it necessary to follow him, as the volume is now published. In conclusion, he observed]—

The other contents of the volume are accounts of researches undertaken at the request of the Association. Notices in answers to queries and recommendations of the same body, and miscellaneous communications. Of these, it is of course impossible to speak now; your time would not permit it. Yet perhaps I ought not to pass over the mention of one particular recommendation which has happened to become the subject of remarks elsewhere—I mean that recommendation which advised an application to the Lords of the Treasury for a grant of money, to be used in the reduction of certain Greenwich observations, the result of which recommendation is noticed in the volume before us. In all that I have hitherto said respecting this Association, I have spoken almost solely of its internal effects, or those which it produces on the minds and acts of its own members. But it is manifest that such a society cannot fail to have also effects which are external, and that its influence must extend even beyond its own wide circle of members. It not only helps to diffuse through the community at large a respect and interest for the pursuits of scientific men, but ventures even to approach the throne, and to lay before the King the expression of the wishes of this his parliament of science; on whatever subject of national importance belongs to science only, and is unconnected with the predominance in the state of any one political party. It was judged that the reduction of the astronomical observations on the sun and moon, and planets, which had been accumulating under the care of Bradley and his successors, at the Royal and National Observatory at Greenwich, since the middle of the last century, but which, except so far as foreign astronomers might use them, had lain idle and useless till now, to the great obstruction of the advance of practical as well as theoretical science, was a subject of that national importance, and worthy of such an approach to the highest functionaries of the state. It happened that I was not present when the propriety of making this application was discussed, so that I do not know whether the authority of Bessel was quoted. That authority has not at least been mentioned, to my knowledge, in any printed remarks upon the question, but, as it bears directly and powerfully thereon, you will permit me, perhaps, to occupy a few moments by citing it.

Professor Bessel, of Koenigsberg, who, for consummate union of theory and practice, must be placed in the very foremost rank, may be placed, perhaps, at the head of astronomers now living and now working, published not long ago that classical and useful volume, the "Tabulæ Regiomontanæ," which I now hold in my hand. In the introduction to this volume of Tables, Bessel remarks, that "the present knowledge of the solar system has not made all the progress which might have been expected from the great number and goodness of the observations made on the sun, and moon, and planets, from the times of Bradley down. It may, indeed, be said with truth, that astronomical tables do not err now by so much as whole minutes from the heavens; but if those tables differ by more than five seconds now, by using all the present means of accurate reduction, from a well-observed opposition of a planet, for example, their error is as manifest and certain now as an error exceeding a minute was, in a former state of astronomy—and the discrepancies between the present tables and observations are not uncommonly outside that limit. The case is doubtful. Errors of observation to such amount they cannot be; and, therefore, they can only arise from some wrong method of reduction, or wrong-assumed elliptic elements or masses of the planets, or insufficiently developed formulæ of perturbation, or else they point to some disturbing cause, which still remains obscure, and has not yet been reached by the light of theory. But it ought surely to be deemed the highest problem of astronomy, to examine with the utmost diligence into that which has been often said, but not as yet in every case sufficiently established, whether theory and experience do really always agree. When the solution of his weighty problem shall have been most studiously made trial of, in all its parts, then either will the theory of Newton be perfectly and absolutely confirmed, or else it will be known beyond all doubt that in certain cases it does not suffice without some little change, or that besides the known disturbing bodies there exist some causes of disturbance still obscure." And then, after some technical remarks, less connected with our present subject, Bessel goes on to say, "to me, considering all these things together, it appears to be of the highest moment (*plurimum valere*) towards our future progress in the knowledge of the solar system, to reduce into catalogues as diligently as can be done, according to one common system of elements, the places of all the planets observed since 1750, than which labour, I believe, that no other now will be of greater use to astronomy"—(*Quo labore nullum credo nunc majorem utilitatem Astronomiæ allaturum esse.*) Such is the opinion of Bessel; but such is not the opinion of an anonymous censor, who has written of us in a certain popular review. To him it seems a matter of little moment that old observations should be reduced. Nothing good, he imagines, can come from the study of those obsolete records. It may be very well that thousands of pounds should continue to be spent by the nation, year after year, in keeping up the observatory of Greenwich, but as to the spending L.500 in turning to some scientific profit the accumulated treasures there, that is a waste of public money, and an instance of misdirected influence on the part of the British Association. For you, gentlemen, will rejoice to hear, if any of you have not already heard it, and those who have heard it already will not grudge to hear it again, that through the influence of this Association what Bessel wished, rather than hoped, is now in process of accomplishment; and that, under the care of the man who in England has done most to shew how much may be done with an observatory, that national disgrace is to be removed of ignorance or indifference about those scientific treasures which England has almost unconsciously been long amassing, and which concern her as the country of Newton and the maritime nation of the world; for the spirit of exactness is diffusive, and so is the spirit of negligence. The closeness, indeed, of the existing agreement between the tables and the observations of astronomers is so great, that it cannot easily be conceived by persons unfamiliar with that science. No theory has ever had so brilliant a fortune, or ever so outrun experience, as the theory of gravitation has done. But if astronomers ever grow weary, and faintly turn back from the task which science and nature command, of constantly continuing to test even this great theory by observation, if they put any limit to the search, which nature has not put, or are content to leave any difference unaccounted for between the testimony of sense and the results of mathematical deduction, then will they not only become gradually negligent in the discharge of their other and more practical duties, and their observations themselves and their nautical almanacs will then degenerate, instead of improving, to the peril of navies and of honour; but also they will have done what in them lay to mutilate outward nature, and to rob the mind of its heritage. For, be we well assured that no such search as this, were it only after the smallest of those treasures which wave after wave may dash up on the shore of the ocean of truth, is ever unrewarded. And small as those five seconds may appear, which stir the mind of Bessel, and are to him a prophecy of some knowledge undiscovered, perhaps unimagined by man, we may remember that when Kepler was "feeling," as he said, "the walls of ignorance, ere yet he reached the brilliant gate of truth," he thus expressed himself respecting discrepancies which were not larger for the science of his time:—"These eight minutes of difference, which cannot be attributed to the errors of so exact an observer as Tycho, are about to give us the means of reforming the whole of astronomy." We, indeed, cannot dream that gravitation shall ever become obsolete; perhaps it is about to receive some new and striking confirmation; but Newton never held that the law of the inverse square was the only law of the action of body upon body, and the question is, whether some other law or mode of action, co-existing with this great and principal one, may not manifest some sensible effect in the heavens to the delicacy of modern observation, and especially of modern reduction. It was worthy of the British Association to interest themselves in such a subject: it was worthy of British rulers to accede promptly to such a request. I have been drawn into too much length by the consideration of this instance of the external effects of our Association, to be able to do more than allude to the kindred instance of the publication of the Observations on the Tides in the port of Brest, which has, I am informed, been ordered by the French government, at the request of M. Arago and the French Board of Longitudes, who were stimulated to make that request by a recommendation of the British Association at Edinburgh. Many other topics, also connected with your progress and prospects, I must pass over, having occupied your time so long; and, in particular, I must waive what, indeed, is properly a subject for your general committee—the consideration whether any thing can be done, or left undone, to increase still more the usefulness of this Association, and the respect and good-will with which it is already regarded by the other institutions of this and of other countries. As an Irishman, and a native of Dublin, I may be suffered, in conclusion, to add my own to the many voices which welcome this goodly company of English, and Scottish, and foreign visitors to Ireland and to Dublin.

Tuesday.—After the sectional business of the day was concluded, a magnificent entertainment was given to the Association by the members of the Zoological Society, at their beautiful gardens. In the evening, the meeting in the Rotundo heard a lecture by Dr Lardner upon steam-engines, particularly as applied to the purposes of locomotion, with details and illustrations derived from the rail-road undertakings already completed, and the great works in progress.

Wednesday.—This day a splendid *dejeuné* was given by the Royal College of Surgeons. In the evening, in the Rotundo, abstracts of the proceedings of the different sections were read by the presidents and vice-presidents. Thereafter, Professor Powell delivered a highly interesting lecture on the undulatory theory of light. Professor Whewell was then requested to state the results of the late *tidal observations.*

The general opinions and facts referred to by the Professor may be collected from his papers read at the Royal Society. In conclusion, however, he observed,—" It was a curious circumstance, that when the attention of the scientific world had been first turned to the tides, observations had been simultaneously begun under the direction of the Royal Society of London, and of the Paris Academy of Sciences. These observations had been continued for some time, until, finding either the problem too difficult, or the perseverance too tiresome, the observers had almost all abandoned their undertaking, without furnishing sufficient data for arriving at a final and valuable result. There were two objects of inquiry with regard to tides; first, the laws of their relation to time; and, secondly, to space. Attempts had been made to ascertain the rate at which the tide travelled, but as yet they had been ineffectual. In June 1834, observations had been ordered to be made by the coast-guard along the coasts of Great Britain and Ireland: returns had thus been procured from 500 different stations. The reduction of these had been only partially effected. In the paper which he held in his hand, he had drawn up his conclusions from these observations. They had done this much, that they had thrown great light on the circumstances attending the meeting of the two tides—the one which ran up the British Channel, the other (we believe) from the Northern Ocean. This was a subject of inquiry equally interesting to the man of science and the navigator. In June 1835, fresh orders were issued to continue these observations along the same extent of British coast, and application for assistance had been made through the ministers of foreign powers. In every case they had been cordially met, and there was not a maritime state in Europe, not one north of the equator, that was not contributing its assistance to this great work: Sweden, Denmark, Russia, Spain, France, Holland, and the United States, had all joined in it. He could not but express his gratitude for the labours of one foreigner, who was now present, he meant Professor Moll of Utrecht, who had furnished them with many valuable returns from the coast of Holland—returns procured by much personal exertion. Monotonous, and calm, and tranquil, as the life of a man of science might appear, his inquiries were sometimes agreeably diversified by an incident of danger. The boat in which the Professor used to make his observations had been on one occasion swamped by the tide which he was examining; fortunately, for the interests of science, the crew were saved. By the next meeting of the British Association he hoped that the inquiries commenced under these favourable auspices will have led to some favourable results. He begged the meeting to bear in mind the general law which he had stated of the progress of knowledge; from arts to phenomena—from phenomena to laws—from laws to causation. It might, it was true, be said, that the cause of the tides was already known to be the attractive influence of the moon; but then, this had been ascertained not by the observation of the tides; these influences had been discovered in inquiries of another character, and in the investigation of phenomena, even in point of space, far remote from the tides on this world, and, so discovered, had been applied to the solution of the phenomena of tides. But he would go further, and say that no one would, even now, be hardy enough to hazard the assertion, if it rested on the unsupported evidence of the tides themselves. We knew, indeed, generally, that these influences caused the tides; but still we had not traced through all the details of mechanical causation the mode in which these influences produce the various phenomena these tides presented. Laplace, the greatest mathematician of his age, had been unequal to the task of these complicated calculations. He could not avoid mentioning, in connexion with this subject, the paper read by Mr Russell, in the Physical section. Mr Russell had successfully investigated the laws regulating the propagation of waves, and had shewn the relation between the velocity with which the wave travels along, and the depth of the water, no matter how long the canal, even were it as long as the Atlantic. Thus, could we ascertain the depth of the Atlantic, we should know the rate of the tide. But might we not reverse the process, and ascertain the depth from the tide? Here there was another instance of the relation of all the sciences, and geology would be indebted for this important knowledge to mathematical and physical investigation; nay more (continued the learned Professor), when the British Association shall meet, 200 years hence, perhaps in this very room—for meet at that period I am convinced it will—the individual who shall then occupy the position which I do now shall turn to the geologist, and telling him the rate of the tide—a rate, perhaps, very different from that at which it travels now—will calculate for him the depths of the ocean; and thus the geologist shall know, with accuracy, the secrets of those depths where no plummet ever sounded, and no line was ever cast, explored by the power of this liquid lever, which the investigations of physical science had placed in his hand; and when they look back to our proceedings at this period, they will, I hope, in returning to next year's meeting, be able to calculate the depths of the ocean back for two hundred years, and thus ascertain for that period the geological changes in the fathomless caverns of the great deep.

Thursday.—An elegant *dejeuné* was given by the College of Physicians. In the afternoon the Lord Lieutenant entertained at dinner a select party of distinguished scientific individuals. In the evening, in the Rotundo, there was a gay and interesting promenade, with delightful music, and all the other agreeable excitements of meetings without formality and without pretension. This deviation from the usual practice of the British Association was universally applauded. Indeed, we cannot help thinking that it would be an improvement, if there was less lecturing and more amusement after the sectional business.

Friday.—This morning a splendid *dejeuné* was given at the Botanic Gardens at Glasnevin. In the Rotundo, in the evening, the Presidents of the different Sections gave an account of the various papers read during Thursday and Friday's meetings.—Professor Babbage then offered some suggestions as to determining the age of peat-mosses by the annular layers of the trees found in them. If, for instance, there be two broad rings separated by a narrow one, it would shew that at some period two years of favourable growth had been divided by one unfavourable year; and it is possible, that by observing similar rings in some very old trees still standing, we might be able to ascertain the period of their growth to have been the same as that of the timber found in "bogs" or mosses.

Saturday.—A General Meeting was held at two o'clock, when the Rev. Mr Vernon Harcourt, as general secretary, addressed the meeting. He apologized for the delay which had occurred, but hoped it would be excused on account of the importance of the subjects which had been discussed before the committee. Invitations had been sent from Bristol, Liverpool, Birmingham, Manchester, and Newcastle, soliciting the Association to have its next meeting in those several places, and what decided the selection in favour of Bristol, was from that city having sent the first decided invitation. Every kind of accommodation had been offered by the public and corporate bodies. A difficulty then arose in selecting the officers of the Association in provincial towns, as they might not wish to undergo the arduous labours and duties necessary to give effect to the proceedings, but there did not appear to be a probability of any inconvenience arising in the present instance. Mr Harcourt then read the several sums of money recommended by the Sections to be advanced for the prosecution of scientific objects in various branches.—It had also been determined to apply to Government to send out an expedition to the Antarctic Regions, for the purpose of discovering the Southern Magnetic Poles. He felt much gratification in announcing that the resources of the Association had increased to a greater degree at this than at any former meeting. He would not waste time in advocating the propriety of the system adopted by the Association, for that system was the most efficacious in imparting knowledge. Mr Harcourt observed, that from the occurrences in the Sections, not only were new subjects remarked, but the spirit in which these discoveries were made was also shewn. The members also derived incitement to new exertions, from the kindness with which they had been treated in every place, and surely in none more than the metropolis of Ireland. The Association offered important means for facilitating discoveries in science, and for awarding the just meed of approbation to the talents of distinguished philosophers. It was said to Dr Black, by a friend, "How do you happen to have made important discoveries, and then stop short instead of completing those inventions, as Priestley and Watt have done." "They have not escaped me," was the reply, "but I am afraid of the reviewers." The reviewers might formerly have had the power to depress merit, but they could not do so any longer. If any man was too modest to give an account of his scientific proceedings to the Section, another member would be prepared to act as a deputy, in order to get that praise awarded to him which he might happen to deserve. There were numerous other points in which the merits of the Association could be considered, but time did not allow him to dilate upon them. Mr Harcourt read the names of the individuals appointed as officers for the year. Treasurer, Mr John Taylor—General Secretaries, Mr V. Harcourt and Mr Baillie—Assistant General Secretary, Professor Phillips—Secretaries, Dr Turner and Mr Yates. Mr Taylor, the treasurer, gave a statement of the funds of the Society. On the 30th of July last there was cash in the treasurer's hands to the amount of L. 509,—in the stocks L. 2361, and unsold copies of works about L. 650. In Dublin the treasurer had received 1228 subscribers, and L. 1750, together with an additional sum of L. 94, for books sold, making the total income L. 5214. The expenses and sums due by the Association were probably L.1000, leaving a clear property of L. 4214. It was gratifying to state that the receipts of the preceding year in Edinburgh were L. 1626, while in Dublin they amounted to L. 1750. It was also very pleasing to be able to state that grants for the advancement of science, of L. 1700, had been placed this year at the disposal of the committee. This sum was distributed as follows: L. 500 for a duplicate reduction of the astronomical observations made at L'Ecole Militaire of Paris; L. 100 for determining the constant of lunar notation; L. 100 for observations on the temperature of the tide; L. 250 for continuing tidal observations at Liverpool and the port of London; L. 100 for the advancement of Meteorology; L. 30 for the continuation of Professor Wheatstone's experiments; L. 30 for reducing to practice Dr Jerrard's plan for solving equations of the 5th or higher degrees; L. 20 to Mr Johnston for completing the tables of chemical constants; L. 30 to Fairburn for experiments on the hot and cold blasts for iron-works; L. 105 for prosecuting researches in British Fossil Ichthyology; L. 50 for researches into the absorbents; L. 50 for examining the sounds of the heart.

Banquet at the University.—The Provost and Senior Fellows of Trinity College gave a farewell banquet, of unequalled splendour, in the evening, to three hundred members of the British Association, in the examination hall of the University. The Earl of Mulgrave arrived at the College at six o'clock, accompanied by his suite; and, shortly after, Professor Hamilton, to whom it had been previously intimated that his Excellency was desirous to confer on him the honour of knighthood, was called for. On conferring on the distinguished Professor this honour, the Viceroy addressed him shortly, but gracefully and eloquently, in the following terms:

> "This is an exercise of one of those prerogatives of royalty, of which I am here the representative, most grateful to myself—most in unison, I feel, with the wishes of that gracious Sovereign on whose behalf I act—most in accordance, I am equally persuaded, with the unanimous opinions of that enlightened people for whose benefit all power is entrusted. This act does not so much confer distinction, as place the royal, and therefore national stamp upon that distinction, which has already been acquired by personal qualifications and individual exertions. On all these grounds, it is with the highest pleasure I now announce to you my present intention, more particularly in connexion with this occasion, where you fill a high official situation in that Association, as members of which we are now here congregated—in the presence of foreigners by birth, strangers to each other in social ties, who are nevertheless drawn together by the irresistible attraction of mutual enlightenment. It is from this brotherhood of knowledge that, as Ireland's Viceroy, I step forward to claim you as her own, and to appropriate to the land of your birth your distinguished reputation. And this I do, sir, because, apart from every other consideration, I recognise in the expansion of intellect, and the development of science, the surest sources of the eternal triumph of truth."

Saturday Evening.—Dr Barry gave an account of his ascent to Mont Blanc.—Professor Babbage made some remarks upon a whirlpool observed at the island of Cephalonia, through which the sea has poured for forty years. He had applied to Lord Nugent, the governor of Corfu, to know whether he was acquainted with the fact, and that nobleman gave him a statement upon the subject, which he would endeavour to report, although, perhaps, not with sufficient accuracy, as he had not taken notes at the time. A hole is seen between two rocks, and an excavated channel conveys the sea-water into a pit, 100 yards round, and four feet below the surface. The sea that enters rushes in with considerable velocity. The water rises in the pit through the sluice, to the height of two feet, and is then discharged through some means not yet ascertained. Mr Babbage said that the waters which disappeared might go into vast hidden receptacles not yet filled up, or else the volcanic agency supplying heat might, as the waters descended into the earth, cause eruptions.—Professor Wheatstone shewed his speaking-machine, and explained the principles upon which this ingenious machine is invented.—The business of the meeting concluded by the thanks of the Association being given to the presidents, &c., and to the public institutions in Dublin for the accommodation granted to them, and especially to Sir John Tobin of Liverpool, who gave one of his fine steam-vessels for the conveyance of members of the Association to and from Ireland.

Thus terminated the fifth meeting of the British Association —a meeting equally interesting and effective with those that preceded it, but out-doing them all in splendour and in displays of national character.

List of Patents granted in Scotland from 12*th June to* 19*th September* 1835.

1835.

June 12. To Richard Phillips, of the New Kent Road, in the county of Surrey, lecturer at St Thomas's Hospital, for an invention of "certain improvements in the process of manufacturing sulphate of soda."

To William Patterson of Dublin, gentleman, for an invention of "a new material for tanning hides and skins," which is also "applicable to other purposes."

20. To Alphonse Humbert Jean Francois Valois of Lyons in the kingdom of France, but now residing at No. 9 Artillery Place, Finsbury Square, in the parish of St Luke, Old Street, and county of Middlesex, gentleman, for an invention of "a certain improvement, or certain improvements, in the mode or method of producing engravings, etchings, or reliefs, on metallic plates, and for producing impressions therefrom, and in the apparatus used in the same."

June 26. To Elijah Galloway of Westmoreland Place, in the county of Middlesex, engineer, for an invention of "certain improvements in steam-engines, which improvements are applicable to other purposes."

To John Losh of 8. Crescent, in the city of Carlisle, gentleman, for an invention of "an improvement in the surface or pattern roll of the machines used in printing calico and other goods, commonly called surface printing machines, and in the mode of working the said rolls."

July 3. To William Kemp of Burslaw, in the county of Stafford, teacher, for an invention of "a machine or apparatus for raising sunken vessels."

4. To Joshua Taylor Beale of No. 11. Church Lane, Whitechapel, in the county of Middlesex, engineer, for an invention of "a simplified and economical steam-engine, which engine may be used for other purposes."

6. To John William Fraser of Ludgate Hill, in the city of London, artist, for an invention of "improvements in apparatus for descending under water, and for raising weights or bodies to the surface of the water."

To Thomas Dunkin of Bourdeaux, in the kingdom of France, but now residing at No. 2. Trinity Place, Charing Cross, in the parish of St Martin in the Fields, and county of Middlesex, late officer in his Majesty's 18th regiment of Hussars, for an invention communicated to him by a certain foreigner residing abroad, for "certain improvements in the mode, method, or system, of obtaining or producing duplicate copies of manuscript writings and drawings, and in the apparatus or machinery used in the same."

20. To Sir John Byerley of Whitehead's Grove, in the parish of St Luke, Chelsea, in the county of Middlesex, knight, in consequence of a communication made to him by a foreigner residing abroad, for an invention of a composition which will effect a considerable saving in the oil and soap used in the woollen manufactures."

27. To James Paterson of Musselburgh, in the county of Edinburgh, net-manufacturer, for an invention of "certain machinery for making herring nets and other nets."

30. To James Kean of Johnston, in the county of Renfrew, in the kingdom of Scotland, machine-maker and engineer, for an invention of an improved throstle-flyer, or a substitute for an ordinary flyer employed in spinning cotton, flax, hemp, wool, silk, and other fibrous substances."

30. To William Evatt Wright of Regent Street, in the city of Westminster, gentleman, for an invention of "certain improvements in tea and coffee urns and tea kettles."

Aug. 7. To Samuel Burrell of Birmingham, in the county of Warwick, manufacturer of gilt toys, for an invention of "an improved method of manufacturing buttons for clothes."

Aug. 17. To Henry Bernard Chaussenot of Leicester Square, in the county of Middlesex, for an invention of "an improved construction of the lamps or apparatus used for burning gas for producing a better combustion of the gas."

22. To Richard Coad of Liverpool, in the county of Lancaster, manufacturing chemist, for an invention of "certain improvements in the means of, or apparatus for, consuming smoke and economizing fuel in furnaces."

24. To Rowland Hill of Tottenham, in the county of Middlesex, for an invention of "certain improvements in certain methods of letter-press printing by machinery."

28. To Frederick Bowman of Great Alie Street in the county of Middlesex, sugar refiner, in consequence of a communication from a certain foreigner residing abroad, for an invention of "an improvement in the process of renewing the virtues of animal charcoal when exhausted or impaired."

To Thomas Hancock of Goswell, Mews Goswell Street Road, in the county of Middlesex, water proof cloth manufacturer, for an invention of "an improvement or improvements in air-beds, cushions, and other articles manufactured from caoutchouc or India Rubber, or of cloth or other flexible material, coated or lined with caoutchouc or India Rubber."

Sept. 16. To William Hale of Colchester, in the county of Essex, civil engineer, for an invention of certain improvements on, or additions to, boilers or apparatus for producing motive power."

To John Dickenson of Bedford Row, London, paper manufacturer, and William Long Tyers of Apsley Mill, in the parish of King's Langley, in the county of Hertford, papermaker, for an invention of "certain improvements in the manufacture of paper."

19. To William Symington of Bromley, in the county of Middlesex, cooper, for an invention of "certain improvements in paddle wheels."

To Andrew Baldrence, chenille cutter, residing in Paisley, for an invention of "a machine for cutting chenille, chenille cloth into chenille thread, for making weft or part of weft, for the shawls now called and known by the names of Chenille, Kamtschatka, moss, and Velours de Soi, 'or one or other of these names.'

NATURAL H

INDEX.

Adie, Alexander, civil engineer, on the expansibility of stone, 207
Agassiz, Professor, his observations on fishes, 331
Air, notice of the effect of mountain, on man, 191
Andrejewskyi, Dr, on the vegetables which are found in the mineral waters of Abano, and in their immediate vicinity, 261
Aphis persicæ, notice on the, 188
Arago, M., on the number of comets in the solar system, &c. 15—on the Egyptian hieroglyphical writing, 39
Arch, Dr Traill on the history of the, 203
Arts, Proceedings of the Society of, 214—Report of the Committee on Mr Beattie's spring door, 313—Mechanical science applied to the, 370
Association, British, at Dublin, proceedings of, 353

Babylon, Dr Traill on the ruins of, 206
Beattie, Alexander, his description of a new spring for shutting a door, 309
Beck, Lewis C., his researches on wines and other fermented liquors, 145
Birds, on the respiration of, 193
Boussingault, J. B., his account of an attempt to ascend Chimborazo, 88
British plants, new and rare found in Scotland, 348

Carpet manufacture, Mr Edward Sang on the recent improvement of the, 254
Chemical and Mineralogical Section of the British Association, proceedings of the, 391
Chimborazo, M. J. B. Boussingault's account of an attempt to ascend, 88
Ciliary motions in reptiles and warm-blooded animals, account of, 114
Clark, Dr, on the application of the hot blast in the manufacture of iron, 205
Comets, M. Arago on the number of, in the solar system, &c. 15—on the light of, and on the means of deciding whether this light emanates from these bodies themselves, or is borrowed from the sun, 21—on the, which will pass its perihelion in Nov. 1835, 31
Connell, Arthur, Esq., on the action of voltaic electricity on alcohol, ether, and aqueous solutions, 159—on taking the Dew-point, 176—on the chemical nature of fossil scales, 300

Copper, native, notice of a large mass of native, 183
Craig, Rev. Edward, on the measurement of the angles of minute crystals, 306
Cunningham, R. J. H., his remarks on the geology of Skye, 208

Daubeny, Charles, M. D., on the degree of selection exercised by plants, with regard to the earthy constituents presented to their absorbing surfaces, 164
Davy, Dr, observations on Faraday's Experimental Researches, 317—on the temperature of fishes, 325
Dew-point, note on the taking the, directly, by the method of Leroi and Saussure, 177
Deluges, Dr Von Schubert on the, of Deucalion, Oxyges, and Noah, 34
Don, David, Esq., on the characters of certain groups of the Personatæ, 108

Earth, temperature of, 143
Earthquakes, notice of, 184
Electricity, velocity of, 179—A. Connell, Esq., on the action of voltaic, on alcohol, &c. 159
Expedition, projected, to Iceland and Greenland, 196

Fish, Dr Fleming's notice of the remains of a, found in coal, 364
Fishes, Mr Parnell's account of some new, found in the Firth of Forth, 209—Dr John Davy on the temperature of fishes of the genus Thynnus, 323—Agassiz on, 331
Fleming, Dr, on the remains of a fish found in connexion with a bed of coal at Clackmannan, 314
Forbes, Professor, his notice respecting the application of the compressibility of water to practical purposes, 36
Formations, Dr Gairdner, his sketch of, 8

Gairdner, Dr, his physico-geognostic sketch of the island of Oahu, 1
Geological and geographical section of British Association, proceedings of, 374
Gold region of the United States, 185
Graham, Dr, on several new or rare plants which have flowered in the Botanic Garden, Edinburgh, 202—his list of plants new to the British Flora, or rare in Scotland, 348
Greenland, projected expedition to Iceland and, 196

Hailstones, Dr L. Cosaire, his acconnt of some remarkable, which fell at Padua, 83

Jameson, Professor, on the birds of the Himmalayan mountains, &c. 208—on a new species of eagle, and notice of the male Lophophorus Nigelli, 212—on new species of Ibis, Tanagra, and Rubecola, 213

Lawrie, Mr James, on the different chronologies of the deluge, 311

M'Nab, Mr James, his account of some of the rarer plants observed during an excursion in the United States and the Canadas in 1834, 56

Mammoth, on the discovery of the remains of the, in India, 196
Martin, Mr G., on the Glasgow and Garnkirk Railway, 48
Mathematical and physical section of British Association, proceedings of, 354
Marcet, M. F., his inquiry into the changes which the atmosphere undergoes when in contact with certain vegetables which are destitute of green leaves, 302
Medical Section of the British Association, proceedings of the, 408
Meetings, evening, in the Rotundo, 416

Nests, edible birds', how formed, 190

Oahu, Dr M. Gairdner, his physico-geognostic sketch of the island of, 1
Observatory, account of the founding of a new grand, for the Russian empire, 71

Parnell, Mr, his account of some new fishes found in the Firth of Forth, 209
Patents, list of, granted in Scotland from the 25th Sept. 1834 to 10th June 1835, 217—from 12th June to 19th Sept. 1835, 436.
Pilot fish, notice of the, 180
Plants, notice of fossil marine, 184—List and description of rare plants flowering in the Botanic Garden, 202—list of, new or rare to the British Flora, 348—Mr J. M'Nab, his account of the rarer plants observed during an excursion in the United States and Canadas in 1834, 56—Dr Daubeny on the degree of selection exercised by, 164
Purkinge, Professor, and Dr Valentin, their discovery of continual vibratory motions produced by cilia, as a general phenomenon in reptiles, birds, and mammiferous animals, 118

Railway, Mr G., his remarks on the Glasgow and Garnkirk, 48
Rogers, H. D. Esq., on the Falls of Niagara, 281
Royal Society of Edinburgh, proceedings of the, 203
Rubecola Tytlerii, Professor Jameson's notice of the, 214

Salmon of the Columbia river, 192
Sandstone, on the imprint of feet of animals upon red, 182
Sang, Ed. Esq., on the manner in which friction affects the motion of time-keepers, 129—on the recent improvements of the carpet manufacture, 256
Scales, A Connell, Esq., on the chemical nature of fossil, 300
Schubert, Dr Von, on the periods of the deluges of Deucalion, Oxyges, and Noah, 34
Sharpey, Dr, his account of the discovery, by Purkinge and Valentin, of ciliary motions in reptiles, &c. 114—additional observations and experiments, 125

Serres, M. Marcel de, his remarks on the question, Does the observation made in the coal-mines of Canada and of Baffins announce a change in the Ecliptic? 64—on the distinctive characters of the dog, the wolf, and the fox, 244
Statistical Section of the British Association, proceedings of the, 412

Temperature, progressive increase of the, as we descend in the crust of the earth, 143
Time-keepers, on the friction of, 129
Traill, Dr, on the history of the arch, 203—on the ruins of Babylon, 206

Wernerian Natural History Society, proceedings of the, 208
Wilson, James, Esq., on the insects of Sutherlandshire, 212–292
Wines, L. C. Beck, M. D., his researches on, &c. 145
Writing, M. Arago on the Egyptian hieroglyphical, 39

BRITISH MUSEUM NATURAL HISTORY

NOTICE TO CORRESPONDENTS.

Owing to the great length of the Account of the Meeting of the British Association at Dublin, Communications, Notices of Books, &c., are unavoidably delayed until next Number.

We regret exceedingly that Mr Hardy's ample and excellent Report of the Proceedings of the British Association in Dublin, did not reach us until the last sheet of our Journal had passed through the press.

PRINTED BY NEILL & CO., EDINBURGH.